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ART. I.—*Anniversary Address delivered before the Alumni of the Medical Department of the University of the City of New York, March* 4, 1872. By D. B. ST. JOHN ROOSA, M. D., of the Class of 1860.

THE character of the audience before which I have the honor to stand, composed as it is of those who are members of the profession of medicine, and of others who represent that body, known in their discussions as the laity, is an admonition that it will not be the time or place in which to enter upon a strictly professional or technical line of thought in what I may have to say. It may be assumed, however, that this is an assemblage of the friends of the medical profession, and that a common field of interest may be found in some of the subjects that arise from the duties of medical men, and which are suggested by such an anniversary as the one we are now celebrating.

The recurrence of the annual meeting of the alumni of the Medical Department of this University very naturally suggests, I think, an inquiry as to the benefits of such organizations. Why are they formed? Of what use is it that we, who have graduated in medicine from the same college, have banded ourselves together in an association with dinners, annual addresses, and treasurers' accounts?

We can all at once see why men who, as boys, have lived in the most intimate social relations for four years, who have sat side by side on the recitation-bench for term after term, and who have finally, almost, if not quite in tears, smoked the pipe of union, and planted the ivy of remembrance on the college green, should unite in class and college associations to live over the days of auld lang syne; but in the three years of a man's life as a medical student, and the few months of them that are spent under the roof of a medical college, there is very little of sentiment, and also very little that one would care to live over again.

Very few of the students of a large class are well acquainted with each other. One term of such a life is often spent at a college in Michigan, another at one in Massachusetts or Virginia, and finally the graduation occurs in New York. So that very few of those who have sat together on the college-benches, and who have jostled and elbowed each other in the crowded wards of the hospital, are even very well acquainted, much less socially intimate.

For these and other reasons that might be given, alumni associations of graduates in medicine cannot have, as their chief object, the renewal of old friendships and college memories. There is, however, as I believe, a great value in such organizations. It is a matter of regret that we have not had them for a longer time. Medical colleges have unfortunately sometimes gotten into the position of representatives without constitutents. They have stood as it were alone. Their graduates have felt that their active interest in *alma mater* was gone when the diploma was obtained. Thus the colleges have lost the aid which a band of loyal alumni would always give, while the profession at large, which all medical colleges serve, have been unduly delayed in receiving the improvements in medical training which would undoubtedly be suggested by men who from years of active practice have acquired ideas that even professors would be glad to adopt.

Who is there who doubts that those honored universities, Harvard and Yale, have been largely the gainers since their alumni have taken an active interest in their affairs? To no less an extent, if the end can be properly attained, will our medical colleges be profited, and through them the whole profession, by alumni associations; for, as I believe, and as I shall attempt to show, it is to the medical colleges that we must look for any great advance in medical education, and I was about to add, in medical science.

Prior to the organization of this association, some six years ago, so little interest had the graduates of this college taken in their *alma mater*, that, although she has an honored career of more than thirty years, and has graduated more than three thousand men, their history remained entirely unwritten, and they exerted almost no influence whatever upon the department. Many of the medical colleges in the country have been furnished with one or more professors who took their degree in the University of the City of New York. In every city, and almost in every hamlet of our land, in Canada, and even in far-off China, Syria, and Hindostan, her graduates were doing successful and honored work, and yet in the city whence they went forth there was not even an annual roll-call. To record this history was one of the objects for which this association was founded. That object will soon be accomplished, for the publishing committee are about to go to press with a catalogue which I trust will be a beginning in the work of rescuing names from oblivion which the world ought not to let die.

But, far beyond any such purpose as this matter of history is the one which I hope will be fully developed in our subsequent career as a society. We should endeavor to increase the activity and resources of our *alma mater* in the field of science and medicine. By thus doing, we shall not only benefit her, but every other medical college in the land, or drive those that are past betterment into a deserved dissolution.

We hear a great deal in our journals and societies of the elevation of the standard of medical education. The phrase has become so hackneyed that it has lost much of its force, and yet all of us will admit that there must be an advance if Medicine is to keep step at all with her sister arts and sciences.

I know of no way in which this advance may be attained except through the medical colleges. To-day, a diploma from one of them is worth all other evidence as to the fitness of its owner to practise medicine, although we are all sorry to admit that even this is not always a guarantee of acquirement. Medical colleges, fond as the profession is of reproaching them, have done more for the scientific education of medical men in our country than all other means combined.

It is claimed on high authority,[1] however, that medical colleges cannot fulfil the task of advancing medical science, or of stimulating strictly scientific researches. It is undoubtedly true that only in a post-graduate course in university laboratories and dissecting-rooms, where there are scholarships and libraries, and all that belong to liberal endowments, may we expect original and independent scientific researches. But certainly a medical college is better able to furnish this course than any other kind of an organization. Moreover, a very large share of all the scientific work that is done in medicine is done by the teachers and *attachés* of medical schools. Of ten papers read before our County Society during our last year, six were from professors in medical schools, and two of the remainder were from avowed clinical teachers. A reference to the catalogue of books published by one of our leading publishers shows that, with three exceptions, these books were written by professors in medical colleges.

I am not here to claim that our medical colleges have come up to that which may be justly expected of them, but such as they are, without them, our societies, journalism, and literature, could not live a day. I think we shall find that medical colleges have as high a standard and do as much for medical science as the profession demands of them. Just as soon as physicians, in such organizations as ours demand more, and show a willingness to assist in carrying out the plans proposed, the colleges will be glad to take great steps forward. But isolated, outside grumbling, attended by spasmodic and impertinent expressions of contemptuous opinions in regard to our best hopes for the cause of medical education, that is, the schools, will only be wasted on barren air.

---

[1] A. Jacobi, Inaugural Address, *New York Medical Journal*, January, 1872.

Why has Harvard Medical School fallen off in her students to the number of one hundred and five? Because her new standard is too high, or because she has no faithful constituency in her alumni, who, having been consulted in the changes made, have promised to sustain them? Did those one hundred and five men stay away of themselves, or did their preceptors and the medical men about them allow them to do so? I am far from saying that all the changes made in Boston should be adopted in New York, but I do say that the Harvard Medical School needs the profession of New England at her back.

There is certainly need for changes in our system of medical instruction. We have outgrown our garments to such an extent that we present almost a ridiculous appearance when viewed in certain directions. Yet what man of us would copy the entire system of medical instruction as it obtains in Germany or England, and incorporate one of those into our plans.

Faulty as our system is, let us calmly see what it has done for us. The average American medical student, at the end of his course of three years, which is so largely voluntary, compares very favorably with the average German who has been engaged in medical studies for five years, in spite of the fact that the American often knows very little Latin and no Greek, while the German knows a great deal of each. Undoubtedly, the German system has fewer defects than ours, but no system will of itself make a scholar or a practitioner, any more than a bad system can prevent a man from being both.

An impartial visitor to the wards of a German hospital, and to those of New York, Boston, and Philadelphia, will tell you that the Americans exhibit quite as good surgery as their transatlantic brethren; and that which we technically call the practice of medicine will, I am sure, not suffer by the comparison.

Again, where was there a better medical corps, one which did a nation more honor, than that which managed the medical department of our army in the late civil war? And to-day will not the medical staff of our army and navy compare well with that of any nation? Where has there been better scientific work than that which has come from Woodward, Otis, Curtis, and others, under the direction of the surgeon-generals? What organization did better medical service in the world's history than our late Sanitary Commission, managed, as to its medical details, by such men as Van Buren, Agnew, Stillé, Harris, and Jenkins?

All of these men were graduates at American medical colleges, and few of them have supplemented the education there obtained, except from the resources of their own land.

A system which has produced such results, and men such as these, should not be wholly condemned. Its defects should be remedied, its virtues amplified, but no revolution should be made, although we may repeat the fact that our course of instruction must be improved. The nation and the world, as the centuries go on, are becoming more critical even as to the culture of medical advisers. It is an undeniable fact that the diseases of to-day are, so to speak, of a more refined nature than those of a hundred years ago. They are those of a civilization which is nearly the same in our young land, so rapidly is knowledge disseminated, as that of peoples whose forests were cleared, and whose fields tilled and cities built, a thousand years ago. As proofs of this statement, let us consider the fact that such affections as the neuralgias, short-sightedness, diseases of the muscles of the eyeball, are greatly on the increase, especially in large cities. Thus we are approximating the civilization of Germany, where myopia is almost the rule among the educated classes. A writer on this disease, some sixty years since, Ware, quoted by Donders, shows that, while short-sightedness was almost unknown among the privates of the English Foot-guards, it was very common in Oxford and Cambridge Universities; in one college at Oxford, thirty-two out of one hundred and twenty-seven, or about one in four, were short-sighted.

Even in Germany, however, in the mountain villages, where the school-terms are short and the studies simple, myopes are rare; while in a class of medical graduates, pursuing a special course at Berlin in 1862, nearly one-half of the Germans wore concave glasses.

The number of publications on nervous disease, on wear and tear, the haggard looks of our overworked men in all kinds of business and in all professions, furnish the evidence of a class of affections of which our fathers knew very little. They were pioneers, forest-hewers, land-breakers; we are the army of occupation in camp and garrison. Our course of medical education needs, then, in these times, more of detail and refinement to meet these enlarged requirements. Not but that we still, and shall always, need bold and skilful surgeons for amputations, resections, and tumors, and for gunshot-wounds, for all the requirements of every-day ills and accidents; but, we must have, besides them, the men who will make minute investigations in diseased tissues, who will study the shades of insanity, those who will adjust the cylindric lens to the astigmatic eye, and perhaps so learn the science of acoustics, that a sound reflector will be made for the deaf.

The course of instruction that was adapted to a time when there was no microscopic anatomy, no auscultation, no ophthalmoscope, no laryngoscope, no otoscope, and no clinical lectures, is certainly not entirely competent to fulfil the requirements of our day. Attempts have been made on all sides to adapt our schools to these requirements, but some of these adaptations have made the state of things worse. We have more lectures, with the same number of hours of instruction. The old preceptor system, under which the medical student actually spent the time in the office or by the sick-bed at the side of his teacher, has fallen into disuse, to be replaced by a summer session which students attend or not, as they please, the fact being that not more than one in four does attend it.

It is true that we now have professors of almost all the branches of medicine, but many of them have no power of participation in the examination for a degree. It does not take much judgment of human nature to decide as to how many students will attend the course of instruction of a professor who has no power to compel attendance. As a practical result, many of the graduates never attend such lectures.

We can readily see how easily all this might be improved, not, as I have already said, by a revolution or convulsive change, but by a natural amplification of the means now employed—make attendance upon the summer course obligatory, allow every one who lectures to examine for the degree, and the chief defects would be remedied. There is one part of the present system, however, for which mild measures will not do. There a radical change is necessary. Students of different degrees of proficiency should not be allowed to attend the same lectures. There should be a class system, and a graded scheme of study. This is a point that requires no argument; it is only wonderful that the present arrangement has had so long a life.

Some of the much-vaunted advantages of the present day, by which men who know nothing of the nomenclature of the diseases of which they get perhaps a peep over the shoulders of a crowd around a sick-bed, vainly attempting to see and hear what only a dozen or two can look at or listen to, also need a radical improvement. Clinical instruction should not be addresses to a mob, but the systematized, Socratic teaching of a limited number, so that the student is obliged to take an active interest in the case before him. Much of the so-called clinical teaching is only by courtesy thus named.

If I were to speak of the improvements that have been made in our present system, I should have a long list to go over. It would be a welcome task to show how the medical colleges have advanced in the last twenty-five years. Since the foundation of this department in 1841, and mainly through that foundation, New York has become a medical metropolis, instead of being what it then was, with relation to Philadelphia, a mere provincial town. Then we had about one hundred students in one school, now we have a thousand in three flourishing colleges, with laboratory, dissecting-room, and hospital advantages which were then scarcely dreamed of even by the most enthusiastic promoter of medical education.

But we, as alumni and friends of medical schools, are chiefly anxious to know what we can do to remedy defects. There are occasions enough for congratulation upon what has already been accomplished.

I am also attempting to show that there should be changes, not the uprooting of the good parts of our American plan, substituting those which are foreign, and which in some respects need as much change as our own. I am aware of that intensely unpatriotic view which looks at all our attempts at science, that are not after the European model, as failures, that one which regards all our efforts in the way of education with contemptuous pity, about as we would view colleges among the Cherokees, and Choctaws, and freedmen; but I, nevertheless, hold to the belief that it remains for this country, and for the city of New York, to develop a system of medical teaching and a race of scientific men which shall have no superiors in the world.

As an illustration of the feelings with which some of our brethren across the sea regard our attempts to cultivate medical science, I may repeat an apocryphal story of what the great strategist of Europe thinks of our military science and art:

Is is said that Von Moltke declined to read any history of the late civil war, because it was an account of the proceedings of an armed mob. It is probable that the Prussian general was never so rude as to say any thing like this, and yet many of us know, if he entertained the same views with thousands of his countrymen, that this would not have been an unnatural expression.

What we do need most, and first of all, in our medical colleges—a need which only alumni can fill by their influence and efforts—are endowments for professorships. The teachers should be free from any taint of desire of large classes, merely that their salaries may be increased. We need more opportunities for special studies and investigations in chemical and physiological laboratories, in the dissecting-rooms, and the clinical wards. We also need libraries and scholarships, in short, what money will bring—money not to be spent on the outside of the cup and the platter, the college building and the lecture-room, but for the support of men who are willing to labor for science, if science can give them their bread-and-butter.

The money that now goes to found new universities in Montana and Nevada should stay in our Eastern colleges, that now have the buildings, but sadly need the internal essentials for making them of use.

The wealthy county, which owes a debt to the medical profession, not by any means paid, is willing to assist in endowing these chairs, and in founding these scholarships, as I have no doubt they would if a proper appeal were made to them, such a one as Chancellor Crosby has so successfully made in behalf of another department. Surely the discovery of the anæsthetic powers of sulphuric ether, an agent whose value cannot even be estimated, deserves some more fitting reward than a monument of brass, in the public garden at Boston. Those who are grateful for that which has robbed the surgeon's knife of nearly all its terrors, can do no greater honor to the memory of Morton, who suggested, and urged upon Warren the use of this blessed agent, than by founding chairs, which shall cause other pain-stilling, death-preventing remedies to be discovered and compounded.

Our lack of opportunity for scientific work in this land has made us medical men a race of translators and imitators, setters forth of other men's ideas, rather than of original thinkers.

But it is not altogether want of means that has prevented us from taking the rank which, as inheritors of the accumulated culture of the Old World, we might have claimed.

The visitor to the ancient University of Leyden, founded when a nation was engaged in a struggle for existence, to which our late war was but mimic strife, who has expected palatial halls and gorgeously-furnished lecture-rooms, must be surprised when he looks upon the humble surroundings of such men as Boerhaave, who did for our science and art what will ever make Dutchmen flush with pride.

And in Berlin and Vienna, as the student of to-day lingers with the mighty masters of those schools, he will see that the means at their command are not those of external surroundings. The most of their advantages are open to us. They are in brief, brains, and objects upon which to use them. We should cease our efforts to become merely fluent discoursers on other men's opinions, we must, by habits of close observation, begin to have opinions of our own. If men with large opportunities are too busy to use them, some of their redundant practice should go to their needier brethren, while a little work is done for the profession. Men with four or five hospital appointments, and who are candidates for more, should resign some of them, and work for science, instead of hurrying to get from one half-accomplished task to another. In our cities, and even in our hamlets, there is many a busy, and wise, and successful practitioner of medicine, who will go down to his grave with facts full of interest locked in his breast, discoveries delayed revelation, not because their possessor is unwilling, but because he is too busy, to tell them.

We need in this country, where pecuniary success does so much more than it ought to secure social position, to beware, as scientific men, of the struggle for crowded consulting-rooms, and an unending round of engagements. The admiration and even the gratitude of the crowd are things of to-day, while the rewards of a devotion to science are eternal.

One of our gifted countrymen,[1] a laborer in another and a higher calling than ours, has read us practitioners of medicine a lesson that we need, in his apt commentary on the old French proverb, *noblesse oblige*. This motto of men of rank and privilege is of itself an argument for professional work in which the idea of pecuniary reward does not enter, and should be one of our rallying-cries.

Even with our present system of education, if each one of us embraced his opportunity, the New York school would soon be quoted, not for the number of its graduates, but, like those of London, Paris, Berlin, and Vienna, for its scientific discoveries.

Brethren of the alumni of this college, let us do something for our school that shall redound to the glory of *alma mater*, and the honor of our land. We may, with our more than two thousand living graduates, become a power in the cause of medical reform. We are more than two hundred even in this city and suburbs, although the Faculty have never aimed to make this a local college. Our organization here should be so strong, and have such a purpose, as to engage the sympathies of our brothers even in the remotest parts of the earth.

In the patient performance of the duties that pertain to such organizations, there is none of the *éclat* attending brilliant professional efforts, but there will certainly be laurels invisible to the multitude, but forever seen by those who honor earnest work for the general welfare.

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[1] Rev. Edward E. Hale, *Old and New*, Sept., 1871.

May I now, leaving this subject of the work of alumni associations in the advancement of medical education, still further trespass on the patience that has heard me thus far, by a brief discussion of some of the relations of medical men to the laity? My excuse for thus abruptly following in the track of my predecessor, our distinguished President, may be found in the fact that, while the medical profession is often criticised, it has few opportunities for answering these criticisms. Hence we are justified in availing ourselves of those which are offered.

The basis upon which physicians estimate their fees is not always as well understood as it should be. Not but that they are cheerfully paid as a rule. Yet a successful or unsuccessful result in the treatment, the propriety of which is not questioned, is often unduly taken into consideration; unduly I say, for I assume that your medical advisers are as careful and as painstaking in their successful as in their unsuccessful cases.

We are often judged by the rules that apply to mechanics, whereas it is evident that their materials are a little more reliable than ours. Cause and effect are somewhat clearer in work upon wood and marble, than in efforts to heal the flesh and purify the blood. Physicians fret and chafe a little under the strictly pecuniary view of the value of their services. They do not consider themselves as mere laborers for hire. I am sure the public at large does not, or we should not have, as one of our greatest blessings, the poor always with us, in the treatment of whose ailments the money question can never with any propriety enter.

The fee is to us, what our code of ethics styles it, an *honorarium*. For, who can estimate in dollars and cents the value of a life saved? Who can set a price to a disabled organ or limb restored to usefulness.

In spite of these theories, which all but suicides are supposed to entertain, how often do we hear a murmur against that *rara avis*, a thousand-dollar fee? The exceptional man who gets it is usually past the middle of a life that has been wholly given up to science and humanity. How often, again, is a fee complained of, because the physician decided that there was no disease present, or because the prescription was unavailing?

The medical profession is far from wanting a change in the present system, by which the honorarium is paid for conscientious effort, no more, no less, whether the result be a cure or failure, to arrest the disease. Yet if we were paid for lungs and throats, eyes and limbs, for health and life, due under God to our efforts, according to their market value as estimated by their possessors, we should have a much greater pecuniary reward than now, even if we received nothing in those cases where the treatment was unsuccessful.

But such a system would destroy our sympathetic relations to our patients, and our calling would lose all of its sacred nature. There are several anecdotes floating about our city, relative to the subject of fees, which may or may not be true, but which, after all, illustrate the paternal relations existing between tried and trusted medical advisers and their patients, such as obtained in the times when people did not change their physicians as readily as they would a garment.

It is said that the late Dr. F., for so many years the chief attraction of many circles of our society, once sent in his annual bill, which was about as large as usual, for medical services rendered the family of one of his most valued friends, when, in point of fact, he had not been in the house professionally during the entire year. The bill was paid as usual, but, when the head of the family met Dr. F., he remarked: "Doctor, I got your bill the other day, but I don't remember that any of us have been sick this year." "Very likely not," answered the bluff man of science; "oh no, but I stopped several times at the area-gate, and inquired of the servants how you all were." For that year at least the good man was paid on the Chinese principle.

The late Dr. S., who was for many years one of the prominent medical men in New York, is said to have once sent in a bill for three hundred and forty-two dollars and ninety-two cents, or some similarly odd sum. This curious bill was also paid, but when the patient met his physician he inquired: "How, doctor, did you ever get that odd ninety-two cents in my bill?" "Oh," said the doctor, "that is easily explained: my grocer's bill was just for that amount, and I knew of no one who would so cheerfully pay it as yourself, and so I made one pay the other."

There has often been in this country, and, judging from what we read, in England also, a chronic difficulty between laymen and physicians in the management of hospitals and similar public charities. In other words, the directors and the medical board do not always get on well together. This difficulty has not arisen from any kind of natural antagonism between the two kinds of people that make up these two bodies. There is practically no such antagonism.

This difficulty, jealousy the one of the other, has arisen, as it seems to me, from the separation of the two bodies, which have a great affinity when in normal proximity, but which become very repellant when separated. Distrust has been engendered by the Chinese wall that has been built up between them in the administration of public charities.

Physicians and laymen should be on the same board and with equal powers, while the special work of each body is done by means of sub-committees. The most of the matters pertaining to the management of such institutions are of such a character that doctors have quite as much interest in them as laymen.

One of the noblest charities that the world has ever seen, the New York Hospital, which owed its origin to Dr. Samuel Bard and Dr. John Jones, has been, as many believe, virtually annihilated, certainly has lost years of its existence, because physicians had no voice, except an advisory one, in its management, and hence could only issue a protest against its destruction. After years of discussion, because the land was very valuable for business purposes, and the funds for its support were not raised, a site that should have been sacred as long as the wharves and warehouses of New York are between the Battery and Twenty-third Street, was leased and the work of the hospital stopped. Now, our only down-town hospital is a little building, formerly an engine-house, at the edge of the Park; while those not fortunate enough, when injured or sun-struck, to gain admittance to the crowded little ward, are *gently* transported by ambulance three or four miles to Bellevue.

I think the statistics, as to the situations in which casualties occur, will show that there was never more urgent need of a hospital down-town, of the high character of the one that is now suffering such an interregnum, than to-day. It is commonly believed in the profession that there never would have been an interruption in the career of the New York Hospital, had the medical force had its full share in its government. Its fame certainly depends upon the character of this latter body, although every one cheerfully admits the high quality of the generous services of its distinguished Board of Governors.

One of the eminent men who form the Board of Governors of the Society of the New York Hospital, in an interesting centenary address, asserts that its grave has been pathetically and fancifully dug, in an article by your speaker, in *Putnam's Magazine.* To this I can only reply that its grave has not only been dug, but its remains so deeply buried beneath the storehouses of Worth and Duane Streets, and Broadway, that probably not one of its present Board of Attending Physicians and Surgeons will be present at its resurrection.

When in all hospitals, as now obtains in some, physicians and laymen meet together as equals to consult in mutual good faith, distrust will disappear, and each class will find that there is no great difference between them after all; that doctors and laity are men of like faults and virtues.

Sir Henry Holland,[1] that eminent physician, whose life reaches over almost three generations, says of our army hospitals of the late civil war that he has never seen them equalled, and he has seen those of most of the nations of the world. All these hospitals were organized and managed, even to the minutest detail, not by practical business-men, with great wisdom in finance, but by physicians.

Our profession is perhaps wholly to blame for the present system of separation between the boards. The management of hospitals has not been wrested from them by a jealous laity, but they have allowed the latter class to assume all the troublesome care and responsibility of the management of the institutions to which they are medical advisers.

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[1] "Recollections of Past Life."

Sometimes physicians seem to have assumed the position of upper servants, not to the poor inmates of the hospitals whose servants true physicians always are, but to boards of direction. It is no wonder, then, that tradition tells, according to the address at the last anniversary of the alumni of the College of Physicians and Surgeons, that the incoming governors of one of our hospitals were instructed to "keep their feet on the necks of the doctors."

Laymen are often greatly exercised over the so-called etiquette among doctors, and we are familiar with the complaint that there is too much ceremony with each other, when a patient desires to change his physician, or when he desires to call in consultation one of the believers in a dogma, an "eclectic," "hydropathist," or the like.

It is probably not as well known as it should be, that we have in the medical profession a system of laws, called the code of ethics, to the observance of which we are just as much bound as is a good citizen to the laws of his country.

If this code be wrong, it becomes our duty to seek to amend it, but, so long as it stands the medical law of the land, we are each one of us in honor bound to abide by it. I think that any careful examination of this code will show that, on the whole, its rules of etiquette are no more burdensome than the unwritten laws of good society. It is only to be regretted that our medical society is as yet in this country so uncrystallized that we need laws to regulate the common proprieties of intercourse among gentlemen.

The occasion does not permit of a full analysis of this code, although I may beg you to give me a few moments upon some parts of it. The code certainly was framed in the interest of the patient. The rule which deprecates even the friendly visits of a physician to the patient of a brother practitioner is an example of the extent to which this protection goes. For how easy is it by a deprecating look or shrug to give a sick friend the idea that, while all that may be very well for *him*, we never employ such a mode of treatment!

Our code says that it is derogatory to the dignity of a medical man to resort to public advertisements inviting the attention of individuals affected with particular diseases, to boast of cures and remedies, to adduce certificates of skill and success, or to perform any similar acts. In short, the code says we shall not attempt to advertise skill. This law sometimes elicits surprise; and yet who would be willing to see the daily newspapers and the dead walls placarded with the notices of the skill of our eminent jurists and theologians in trying cases and in converting souls?

If our abilities as healers of the body were to be generally advertised, the difficulty sometimes complained of, of not knowing to whom to go for the treatment of serious disease, will be by no means remedied, for the practice of medicine would then be a race in which the cunning user of printer's ink would win. It is certainly wise, and in no respect derogatory to the highest character, for a man to advertise that he has a thousand bales of cotton or a cargo of tea to sell; but where is the honest man who shall presume to offend the God who cureth our diseases, by advertising that he is a sure healer of the ills of human flesh?

The medical profession is sometimes thought to be very unbusiness-like because it obeys that part of the code which forbids its members from holding a patent for any surgical instrument or nostrum, or from in any way keeping secret from the brethren the virtues of a remedy.

Let me suppose that a physician in San Francisco discovers a remedy for a blighting disease, and it is well established that he has alleviated or cured many in that city who are suffering from it. If a loyal man, he at once, through the medical press, informs his brethren throughout the world of what he has learned, and we in New York are soon enjoying the benefits of his knowledge, which is now the common property of the profession. Now, let us reverse the picture, and suppose that our San Francisco friend would not tell his secret except to those who visited him personally, and paid for it, must we in New York go unrelieved because we are not able to go to him? This case, though an extreme one, covers the whole ground of argument on this question. The spirit of this wise provision of our code gave us ether as an anæsthetic in New York almost as soon as Morton and Warren had used it in Boston; and chloral was quieting pain in the sick-chambers of our metropolis immediately after Liebreich had tested its virtues in Berlin.

Systematic violation of this article of our code, which places the knowledge of one within the reach of all, would soon sweep our libraries and medical press from existence, and transform a liberal profession into a number of mercenary trades-unions.

Our profession has lately been brought to blush for one of its members, who formed a company for the exclusive sale and charlatan-like advertising of a drug which is to banish cancer from among the scourges of our race. If it were really what the calm judgment of the profession, that has fairly tried it, shows it not to be, cundurango would need no company for its sale, other than the individuals and firms that now faithfully dispense the well-tried medicines of our pharmacopœia.

It would be better far that one medical discoverer should go down to his grave in the dregs of poverty, than that, by his becoming enriched from the exclusive sale of ether and chloral, thousands should be deprived of the mitigations of anæsthesia and anodynes. The duty of our profession is plain: no trade secrets—no patent rights in things medical.

The question as to whether we should consult with men who honestly believe, or seem to, that cold water is a panacea, or with those who, discarding all the results of chemistry and physiology and pathology, believe that symptoms, and symptoms alone, are worthy to be studied, and are then to be healed by mysterious drugs redolent of the dark ages, whose potency increases with their dilution, and that diseases are cured in consonance with what they are pleased to call a law, that like cures like, is a delicate one to argue in the face of a New York audience. Perhaps some of my friends are ready to ask, should they who have so simplified the treatment of disease, so that every one with a book and numerous phials of granules is a competent medical adviser, consult with the murdering old fogies who blister and leech, and give mercury and opium, and whom they are pleased to call *allopaths.*

I do not propose to discuss this question at all, but I beg to state why we in the regular profession reject the name of "allopath," which the dogmatists have attempted to fasten upon us, and to show what we really are, what are our aims, what we have achieved, as well as the unreasonableness of those who call us illiberal; and then leave you to decide where the name of physician, the only one we claim, really belongs. Our profession does not believe in any one dogma; hence we cannot be disciples of "*de curatione per contraria*"—nor of *de curatione per similia.* These are all fancies with which our practical and catholic school of medicine has nothing to do. They are finely-spun theories woven in the days when pathology, and chemistry, and physiology —in other words, exact objective examinations on the living and dead subject, and in the chemical laboratory—had done nothing for the cure of disease; when men were writers not on what they had seen on the body, but of what they imagined in the dreams of their study, and when they were readers of musty tomes, instead of being in the wards by the source of all medical books, the patient.

We of this age should have no interest in these theories; we are simply physicians, as yet unaware of the fixed but mysterious laws that others claim—who now, as always, will use any appliance or any remedy that, judging by experience in their use, or from well-settled mechanical, chemical, or physiological principles, will cure disease. We reject no one from our ranks who acts in this manner. We allow the largest liberty in the employment of remedies, although we cannot believe that cold water is the only means of treatment; that no remedies are of value except they belong to the vegetable kingdom; or that the value of all drugs is increased by attenuation and dilution, and that they all act in accordance with the so-called law that like cures like. In short, we believe with one of our eminent physiologists, Dr. B. W. Richardson, of London, that "dogmas in medicine ought henceforth to be allowed no moment of life; every step of advancement in curing disease must be a single step, proved by its own excellence, based on its own merit. It must be like a chemical experiment, the details of which are known, and are susceptible of being tested and demonstrated by every competent practitioner."

The regular profession, the true one, is certainly to be found with us who claim the name. She is to-day the custodian of all the great hospitals of the world, from Constantinople to London, from New York to Pekin. Even in Leipsic, where Hahnemann once flourished, and whence he was to revolutionize the medical science of the world, a statue is the principal evidence that the pretender ever existed. The student of medicine, who visits the Old World for instruction, will find that not one of their great schools has swerved from the ancient but progressive faith. He will return a disappointed but a wiser man, if he expects to find a school where the dogmatists are teachers.

The literature of the practice of medicine, from Hippocrates down to our own Watson and Niemeyer, is ours, and does not contain a line of comfort for absurd theorizers about the principles upon which remedies act.

The anatomical and physiological researches of Galen, Valsalva, Eustachius, Corti, with hundreds more down to Helmholtz, Richardson, Flint, Dalton, and Draper, of the nineteenth century, form one of the quarterings on an untarnished shield.

The surgical achievements of Pott and Larrey, of Syme and Simpson, of Cooper and Dieffenbach, of the Posts, Parker, and Mott, of Stromeyer and Billroth, of Langenback, Erichsen, Fergusson, Bigelow, Sims, Van Buren, Pancoast, Sayre, and Gross, are the glory of the regular profession, in which the adherents of dogmas have no part.

Pathology, the offspring of modern time, is one of her children, and she points with pride to such names as Rokitansky and Virchow, Stricker, Robin, Claude Bernard, and Beale.

In special researches, where, outside of her ranks, can be found the peers of Mackenzie, Arlt, and Bowman, Wilde, Tröltsch, Gruber, and Politzer, Donders, Hebra and Wilson, Emmet and Leaming, and what body but the regular profession is the guardian of the memory and fame of Albrecht Von Graefe?

Thus I might go on, and the recital of names would be but the enumeration of triumphs. They belong to a profession which, though often stigmatized as illiberal, has always freely given to the world the results of her labors.

To that catholic profession all true laborers for science are welcome. There is room enough beneath the folds of her standard for all the diversities of honest opinion. She means to be liberal and humane in her dealings with error and ignorance, while her honors are reserved for the wise and loyal.

Art. II.—*Resections of the Maxillary Bones without External Incision.*[1] Illustrated by the few following cases. By D. H. Goodwillie, M. D., D. D. S., New York City.

There is perhaps no part of the osseous system that is more prone to take on pathological conditions than the maxillary bones. This may be accounted for by their great vascularity and numerous sources of irritation, especially by diseased teeth. When these conditions occur there are few points in surgery that require more careful judgment and skill to get at a correct diagnosis and treatment.

The earliest successful attempt at removal of a portion of the superior maxillary for a tumor was made by Akoluthus, a surgeon of Breslau, in 1693. Following him in the last century were Desault, Garengeot, Jourdain, White, and others.

So far as known, the first resection of the entire superior maxillary was made in 1820, by Dr. Jameson, an American surgeon.

Four years later (1824) Dr. David Rodgers, of New York, performed the same operation.

In 1826, Prof. Lizars, of Edinburgh, showed anatomically how it could be possible to remove the entire superior maxillary, and in December, 1827, carried his proposition into effect. There was so much hæmorrhage that the operation was discontinued, notwithstanding the carotid was ligated. In May, this same year, M. Gensoul, of Lyons, removed the entire maxillary bone for a boy seventeen years of age.

These operators were soon followed by Syme, Mott, Fergusson, Dieffenbach, and others.

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[1] Read before the Medical Society of the county of New York, June 24, 1872.

From this time the operation became an established one in surgery.

Sir William Fergusson made some valuable modifications by making the median external incision, thus producing less disfiguration of the face.

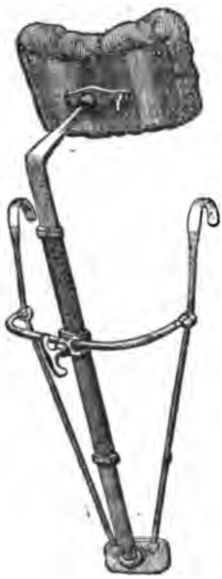
This certainly was a step in the right direction, as heretofore very little attention was given to this.

The first operation on the lower jaw is reported to have been performed by Anthony White in 1804. In 1812 by Dupuytren; 1821 by Mott and Graefe; in 1824 by Sir P. Crampton, and at the present the operation is quite common.

It is not my purpose at this present time to give a description of the many diseases that involve the maxillary bones, or of the several operations for their removal, but to present in the following cases a method of treatment by extirpation without external incision.

In the operations within the mouth much of the success depends on the dexterous use of a few efficient instruments, and, in order to make the following operations intelligible, it may be necessary in the outset to describe the most important instruments used.

FIG. 1.



It is quite desirable to perform all these operations in an operating-chair with a suitable head-rest. This may not at all times be convenient to have, but I consider it extremely necessary to have a head-rest. The necessity of this will be seen when the position of the surgeon in performing these operations is considered.

Fig. 1 represents a portable head-rest (O. C. White's) that can be adapted to almost any chair or even a table. It has movements in all directions, and can be quickly and securely held in position by turning a single thumb-screw. The head-piece is quite concave, so as to fit and firmly hold the head. It can be folded up so as to occupy but little space for transportation.

FIG. 2.

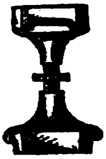
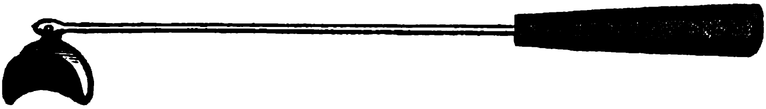


Fig. 2 represents a mouth-gag that I have had constructed to keep the mouth open. The two ends are armed with elastic rubber, and the instrument can be made long or short by turning the thumb-screw seen in the middle.

FIG. 3.



In Fig. 3 is shown a cheek-holder, so made with a long handle that an assistant can use it at a distance, and at any angle.

FIG. 4, *a*.

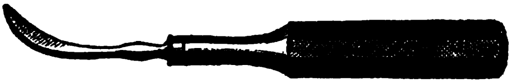


FIG. 4, *b*.

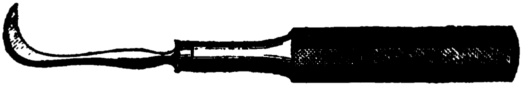


Fig. 4 shows two periosteum-denuders that are so constructed in shape and size as to denude the periosteum in any case without cutting it, and to reach all positions. A third one is also used similar to the first one (*a*), with only this difference, that the end of the instrument is broader.

FIG. 5.

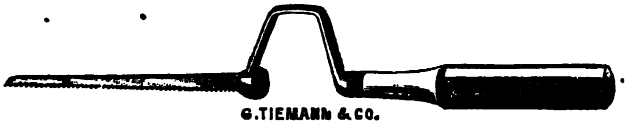

G.TIEMANN & CO.

Fig. 5 represents an oral saw that I have devised and used in operations on the maxillary bones. It consists of a handle to which is firmly fixed a U shank to take in the cheek or lip during the operation. On the other extremity of the shank is a square socket to hold saws or knives of different sizes, these being fastened by a thumb-screw. The socket being square allows the saw or knife to be turned and cut in four directions.

It is only necessary to unscrew the fastening a little in order to turn the saw in the desired direction. The saw being in direct line with the handle can be easily guided. The back of the saw is thin, while the teeth are broad, thus giving it free action.

Among the other instruments necessary are an alveolar lancet, one half-dozen sponge-holders with suitable sponges, and a syringe with a long, curved nozzle, the end pierced with holes, so as to effectually wash out the nasal cavity. Where the soft parts are saved, a long-handled needle, such as used in vesico-vaginal fistula, will be required, along with some other instruments for twisting silver wire, etc.

Three pairs of small excising forceps for necrosis. It might be well to have cautery irons of two or three sizes at hand. I, however, very seldom use them.

Undoubtedly the best position for the surgeon is to the back and over the head of the patient. The head of the patient should come up to the breast of the operator, where it is better under his control, and he can see all parts of the mouth with a good light, and also be less liable to have the blood thrown on him by the expectorations of the patient. If the surgeon is ambidexter, it facilitates the operation very much, otherwise he may have to occasionally change positions.

The following cases will be sufficient to illustrate the resection of the superior and inferior maxillaries by a new method. In all these cases the periosteum was saved.

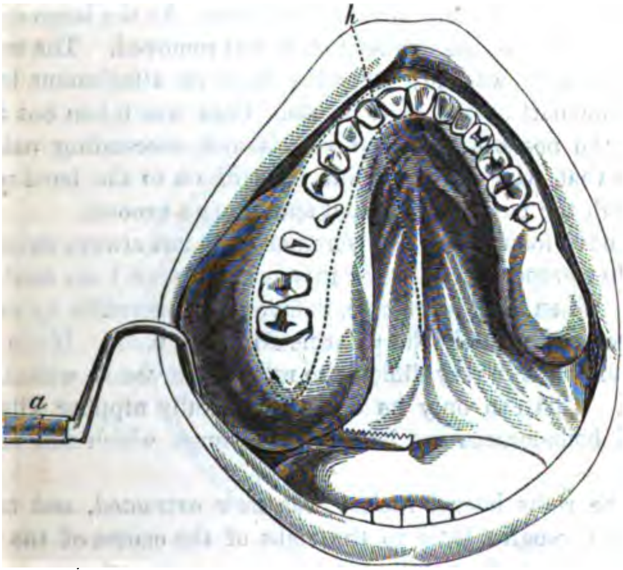
Osseous Tumor of the Right Superior Maxillary.—Mrs. B., aged forty-four years, has always enjoyed good health. About six years ago she noticed that the alveolus of the right superior maxillary began to enlarge, and has gradually increased to the present time. In size and shape it very much resembles a hen's-egg, the large end presenting posteriorly. It extends antero-posteriorly from the right superior canine to the internal pterygoid process, laterally from near the centre of the palate to the maxillo-malar fossa forward to the canine fossa, and to a slight degree into the antrum of Highmore.

The mucous membrane over the surface of the tumor appeared a little lighter in color than normal; this, no doubt, was due to the tension on it by the parts below. In the surface of the tumor could be seen the fangs of the first and second molars. The canine and bicuspids were not decayed. First bicuspid and canine retained their normal position in the jaw, but their crowns were somewhat buried in the tumor. The crown of the second bicuspid could all be seen above the surface of the tumor, but the whole tooth was raised out of its natural position, and thrown inward about one-half an inch. One of the roots of a molar was lying longitudinally in the soft parts on the surface of the tumor.

She has experienced no pain, or discharge from the mouth or nose, during the long period of its growth; from its apparent firm texture, together with the excellent health the patient has always enjoyed, there appeared no doubt of its benignant character.

She desired it removed, as from its great size it gave her great discomfort by preventing mastication, and speech being somewhat impaired.

FIG. 6.



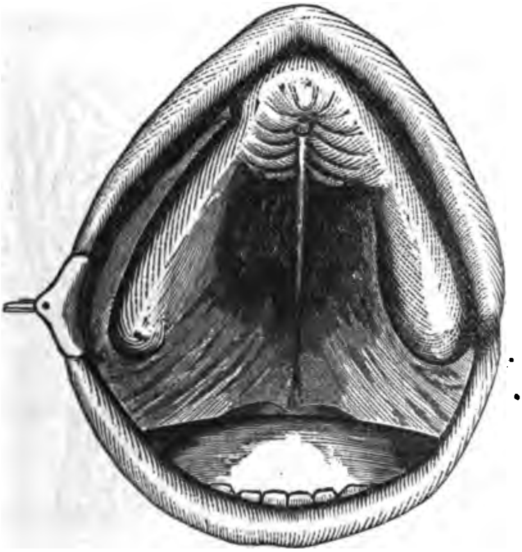
In June last, in presence of Profs. T. M. Markoe, H. B. Sands, W. A. Hammond, H. D. Noyes, A. B. Crosby, and others, the patient was placed in an operating-chair, and, when fully under the anæsthetic, the head was thrown back in position, and the mouth kept open by the gag placed between the back teeth of the opposite side. Taking my position to the back, and over the head of the patient, I placed a sponge cut so as to completely fill up the passage to the throat, and held in position on the soft palate by a sponge-holder to prevent the blood passing into the throat during the first part of the operation. The patient was only allowed to breath through the nose, which she could well do. No external incision of the face was made.

Two internal incisions were made from behind on the posterior prominence of the tumor, one-half inch on each side of the fangs of the molars forward to the left central incisor (as seen by the dotted lines in Fig. 6). The periosteum was denuded with the periostomes by commencing externally at the left central incisor, and passing backward over the tumor to the internal pterygoid process, and upward to the malar bone; then internally from same point over the tumor on to and a little past the centre of the palate. As the large sponge could now be of no further use, it was removed. The tensor-palati muscle was now denuded from its attachment to the posterior part of the hard palate. Care was taken not to injure the posterior palatine vessels and descending palatine nerve that pass at this point forward on to the hard palate through the posterior foramen and along a groove.

I have found that this foramen does not always exist, but in what proportion of cases this might occur I am unable to say. When this is the case, it favors the operation by saving the vessels to supply the palatine flap with blood. If the foramen exists, it is very difficult to release the vessels without injury. This can only be done by carefully nipping with the small bone-forceps the bony ring through which the vessels pass.

The right lateral incisor was now extracted, and by its socket through a little to the right of the centre of the hard palate (Fig. 6, *h*), so as to save the vomer, a section was made with the oral saw, thus dividing the superior maxillary bones. This saw was now changed for one much shorter, the teeth of which was changed to a different angle, so as to allow the cheek to go into the **U** shank, and also let the saw play freely. This section was made up between the tumor and the internal pterygoid process (Fig. 6, *a*) to the malar bone, then forward through the canine fossa, dividing also the inferior tubinated bone, to meet the other section at the right ala nasi. After the saw had entered the antrum in this last section, the handle of the saw was advanced more rapidly than the point; this prevented the point of the saw from piercing the vomer, which I desired to save. By these two sections the tumor with some adjacent bone was removed clean. After the slight hæmorrhage had stopped, the soft parts were closed by seven silk sutures. In the evening gave sulph. morph. gr. $\frac{1}{4}$. The wound was dressed twice a day for the first week, in order to keep the parts free from nasal mucus. I used a wash of glycerole of thymol, x gr. to the oz. of tepid water, applied with a syringe by introducing the long, curved nozzle into the right nostril.

FIG. 7.



Nourishing liquid food was taken for two weeks through a glass tube, passed well back into the mouth, to prevent the food from touching the wound. On the fifth day, four of the intermediate sutures were removed, and the remainder on the following day.

On the seventh daya very small opening appeared; the edges were pared, and the part soon healed.

An impression of the parts was taken three months after the operation, and Fig. 7. is an exact representation of it.

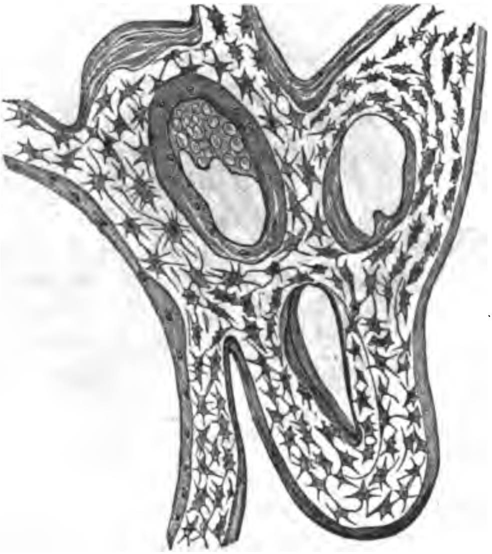
It is now a year since the operation, and the palate appears as firm on the resected side as on the other.

It is the opinion of several surgeons that have examined it, that there is a new deposit of bone. The muscles of the soft palate are not contracted, and the uvula is in normal position.

Pathological Appearances.—On making a section of the tumor through the longitudinal direction of the teeth, there was to be seen the following: At the apex of the second molar tooth there was a small, soft cyst, containing some pus, and, for a short distance surrounding this the bone appeared quite cancellated, but the rest of the tumor was quite dense in structure.

The pulps of the canine and first bicuspid had still some vitality, but that of the second bicuspid was dead. The pulp-chambers were decreased in size by a deposit of osteo-dentine to their walls, slight hypertrophy of the cementum on the fangs. A large nerve entered the tumor on its buccal side.

FIG. 8.



The microscopical examination of this tumor, as made by Dr. J. W. S. Arnold, and as shown by his drawing, Fig. 8, is "composed of cancellated tissue almost entirely. The outer edge of a thin layer of more compact bony tissue. In the spongy part a small amount of soft marrow, containing the usual constituents of fœtal marrow, i. e., medulla-cells, and myelo-plaxes with oil-globules."

The most probable cause of this growth appeared to be the irritation of the periosteum by the decaying roots of the teeth: Nature's effort to throw off every thing that becomes abnormal to her; and so when a tooth loses its crown (it being of no further use in the economy), the roots, after the death of the peridental membrane, rise up to the surface, and occasionally loosen and fall out.

As has been stated, one of the roots of the molars lay embedded in the soft parts on the surface of the tumor, while the other roots had very little attachment to the bone, and really became foreign bodies stimulating the periosteum to produce an excess of bone.

The following case will serve to illustrate from a surgical point, and also give pathologically, a rare and remarkable heterogeneous development of the dental tissues. I have been able to find only three similar cases recorded. The first occurred in the practice of Sir William Fergusson (Tomes "Dental Surgery," p. 224). The second was presented to the French Academy by Dr. M. Forget, in 1859. The third was reported to the Odontological Society of Great Britain, in 1862, by W. A. Harrison, F. R. C. S.

OSTEO-DENTAL TUMOR OF THE INFERIOR MAXILLARY.

Miss A. C., aged twenty-three years, of slight figure, and in somewhat anæmic condition, sought advice in regard to an enlargement of her lower jaw, that had given her trouble from childhood. From inability to masticate her food, her health had suffered much. She complained of great pain on closing her jaws, and much tenderness at all other times.

Externally there appeared a swelling on the right side of the inferior maxillary anterior to the ramus.

On opening the mouth, a tumor was seen to extend from near the ramus forward to the first bicuspid tooth. On the surface of the tumor there appeared embedded a portion of a crown of a deciduous molar.

None of the permanent molars or the second bicuspid had made their appearance. The first bicuspid, with all the other anterior teeth on that side, were well developed. The tumor involved the alveolus, the top of which was quite broad, while the base of the jaw appeared in normal condition. From the upper teeth closing on the tumor, and by the irritation thus produced, there was a slight discharge of pus. Examining this portion of the deciduous molar seen on the surface of the tumor (Fig. 9, *a*) with a probe, it was found to be quite firmly embedded in the bone, that was very much increased in size around it. By this examination I was strongly of the opinion that this was an osteo-dental tumor, i. e., an hypertrophy of the anatomical elements (with the exception of the enamel) of a tooth; there being also hypertrophy of the surrounding bone.

I was led to this diagnosis from the following prominent points in this case: The process of second dentition was imperfect, there being no teeth posterior to the first bicuspid, the space now occupied by the tumor.

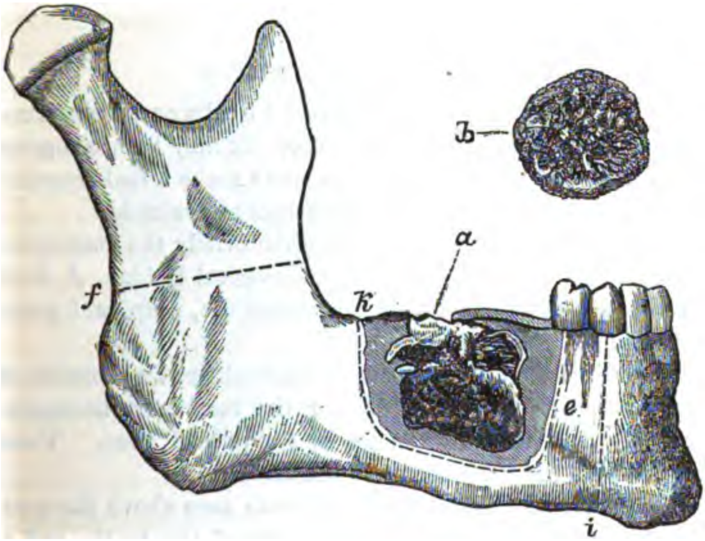
She has distinct recollections of having while quite young a deciduous tooth extracted from that side and none since.

The opposite side has three well-developed teeth in the corresponding space. The small fragments in the surface, resembling the portions of a deciduous molar, is undoubtedly the enamel of a tooth, and not bone. When the probe was passed around this, they were fastened firm in what seemed to be some hard, rough mass below.

This trouble commenced at an early age, and was localized at this point from the very first. From all appearances it was considered benigned. As the base of the jaw underneath the tumor appeared in normal condition, I did not feel justified in removing it, and made the following excision of the tumor. As the operation was made in the country, the head-rest was attached to a common table. The head was not thrown back so much as in operations on the superior maxillary. No external incision was made in this case.

Two incisions were made one-half inch apart from the ramus to the canine tooth on each side of the top of the tumor. The periosteum, denuded from the part to be taken away, and the first bicuspid, was extracted. An oral saw of sufficient length was placed in the socket in right angles to the **U** shank. The cheek-holder that had been used thus far in the operation was now removed. The section with the oral saw commencing behind the tumor (Fig. 9, *k*), close to the ramus, through the jaw, to within about one-half inch of its base, then forward to the first bicuspid, and upward to the surface by its socket (Fig. 9, *e*). The inferior dental artery was secured by torsion. On examination, the base of the jaw was found quite healthy. The periosteum was closed by two silver sutures. As the tongue and cheek acted as splints, more was not necessary.

FIG. 9.



PATHOLOGICAL EXAMINATION OF TUMOR.

The examination of the tumor proved the diagnosis to be correct. On the top of the tumor (Fig. 9, *a*) was seen a portion of the enamel of the crown of a tooth, very much like a half-decayed deciduous molar. The part below this in external appearance was rough on the surface, and seemed dense in structure, with the exception of a concave surface on the bottom of the tumor, which was quite porous, and sticking out from this were a number of spines between which are minute openings into the centre of the tumor for the passage of the vessels of the pulp.

Alongside this were two other concave surfaces, not so deep, but dense and somewhat smooth. These were produced by the cusps of a molar tooth found below the tumor at this point, but very much destroyed by the pus in which they were found. This was probably the cusps of the first permanent molar, and the tumor was the last deciduous molar presenting this abnormal appearance. No other teeth were found.

On making a section of the tumor, there was seen columns or spiculæ running from the circumference to the centre, forming quite a net-work in which the pulp was held, so that, instead of having one pulp-chamber, there were many (Fig. 9, *b*).

MICROSCOPICAL APPEARANCE.

A dental germ that is compelled to take on an abnormal position may have different sequelæ. It may be heterogeneously developed, as in this case, it may make a mal-eruption, or remain encysted, or die when partially developed.

It may be well just here to describe briefly the anatomical structure before considering the pathological lesion. A tooth is composed of enamel, dentine, cementum, pulp, and periodonteum.

The microscopical structure of *enamel* (crown) consists of long, slender, solid, prismatic, for the most part hexagonal fibres of phosphate, carbonate, and fluate of lime. These fibres radiate from the centre to the surface.

This covers that portion of the tooth seen above the gum. The *dentine* makes up the great body of the tooth, and is found between the pulp and the enamel and cementum.

Dentine is composed of numberless tubules about one ten-thousandth of an inch in diameter. Their course is from the pulp to the enamel or cementum. Chemical difference between enamel and dentine is in the absence of the fluate of lime.

The *cementum* is that portion that covers the fangs. It closely resembles in character the osseous structure. The microscope demonstrates the existence of Haversian canals and corpuscle of Purkinje (osteoplasts of Robin).

The *dental pulp* is a soft, translucent, reddish-white substance, enclosed in the centre of the tooth, called the pulp-cavity. It is made up of delicate connective tissue, dental nerve, artery, and vein, and is extremely sensitive and vascular. This is enveloped in a very delicate and apparently structureless membrane, adherent to the walls of the pulp-cavity, and continuous with the alveolo-dental periosteum.

Peridental membrane, that covers the fangs of the teeth, differs very little anatomically and physiologically from membranes of its order quite prone to take on pathological conditions, perhaps more so than any membrane of its class.

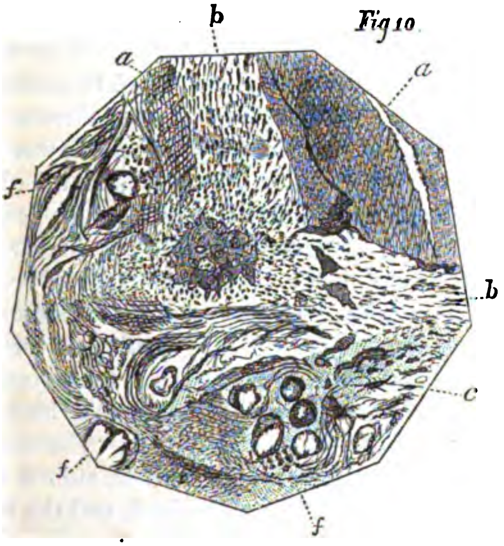


Fig. 10, drawn from a section of the tumor, made by Dr. Arnold, represents the dental tissues in a deranged and distorted condition. The internal structure was very much fenestrated.

a, *a*, represents the enamel at the top of the tumor penetrating into fissures or depressions in the mass below. There are some instances where a portion of enamel was quite isolated by dentine or cement.

b, b. Here is represented the dentine, passing from the centre in two directions. In one instance passing between two layers of enamel, and in the other between enamel and cement.

c. The cement is here to be seen recognized by the presence of lacunæ.

f, f, f, represents the fenestra, once occupied by the dental pulp. Around some of these may be seen the distorted dentinal tubuli.

EXCISION OF PART OF INFERIOR MAXILLARY FOR NECROSIS.

William N., aged twenty-seven years, born in New York. In 1863 came home from the army with intermittent fever, and, about two months after his arrival in the city, a swelling appeared on his right cheek, opposite the first inferior molar, from an injury received some time before. This was opened, and he states that the opening continued to discharge for six years, during which time he had extracted three sound teeth.

On his first visit to me, in 1869, his cheek was very much swollen, and he was unable to open his mouth. I lanced it, and evacuated a large amount of pus. At his next visit, the swelling had nearly all gone, with still some discharge through the old fistulous opening in the cheek. The border of this opening had the characteristic appearance of necrosis.

On passing the probe into this opening, it went into the mental foramen, and passed backward. I next made an examination of the mouth, and discovered that he had lost the second bicuspid, first and second molars. The wisdom-tooth still remained, and was not decayed, but, after a careful examination, I found the dental pulp dead, and the tooth somewhat loose. I extracted it, and hanging to the apex of the fang was a very large, soft cyst, filled with pus. There was a communication from the socket of this tooth through the bone to the external opening.

I washed it out, by means of a syringe, with carbolic acid and water (x gr. to the oz.), placed a tent in the external opening, and directed him to return should there be any further trouble.

I considered that this wisdom-tooth gave all the trouble, and, now that it was removed, recovery would follow. Some three months afterward he returned, and reported that he had still a constant discharge, with much pain.

In passing the probe, both from the external opening and from the socket of the extracted third molar, I discovered extensive necrosis of that part of the jaw between the ramus and the second bicuspid. His general health was very much reduced, by this long drain on his system and inability to properly masticate his food.

I at once determined to remove all that portion of the jaw between the ramus and the canine tooth. After putting him in as good condition as possible with tonics and nourishing liquid food, the following operation was made: When etherized and in position, I completely separated the periosteum from the bone, commencing at the symphysis, and extending to within an inch of the temporo-maxillary articulation. There was very little hæmorrhage until the inferior dental artery was reached. An incision one-half inch in length was made in the periosteal flaps opposite the ramus, to allow the instruments to pass around the bone.

The canine was extracted and the jaw divided with the oral saw from below upward through the socket of this tooth (Fig. 9, *i*). The necrosed portion was now firmly held with a spicula-forceps by an assistant, while the other section was made half-way between the ramus and the articulation. The saw was passed behind the bone, and the section made forward (Fig. 9, *f*). Stopped hæmorrhage from the inferior dental artery by a ligature. Closed flaps with four silver sutures. A tent was placed in the fistulous opening, to allow any discharge to pass freely out.

It is my practice to secure this drainage by passing a seton in the most favorable part. The plaster-of-Paris splint, that was made before the operation, was applied and firmly secured.

There being good articulation of the teeth of the opposite side, it was not necessary to use an interdental splint. This case had a good recovery, although somewhat delayed by the weakened condition of his system.

From demonstrations made on the cadaver, I am persuaded that disarticulation of the lower jaw may be performed without external incision, in the following manner: If the periosteum is to be saved, denude it from the bone to the joint. With an instrument, similar to a gum-lancet, sever the attachments of the internal and external lateral ligaments. Then make the anterior section of the jaw with the oral saw, as before described.

A knife, one inch in length, flat upon one side, and convex on the other, with cutting edges on one side and end, is placed in the socket of the U shank of the oral saw-holder, with the edge presenting forward when in position for use.

Pass the knife behind the ramus and up to the articulation, and let it be brought upward and forward, at the same time rotating the handle somewhat, with occasional depressing and elevating it, thus severing the capsular ligament. The knife may be carried over the sigmoid notch and coronoid process, detaching the temporal muscle.

Before the knife enters the articulation, elevate the anterior portion of the jaw, and depress when passing over the coronoid process. This operation is done with greater ease in the young than in the old. Should it be necessary, in order to reach the joint, an external incision may be made through one or both of the lips in the median line.

PLASTER-OF-PARIS SPLINT.

The following is a simple and efficient method for making a plaster-of-Paris splint for the lower jaw. Take a piece of pasteboard or a whole newspaper folded to the size and cut to the form and dimensions (7 × 6 × 9 inches) seen in Fig. 11.

FIG. 11.

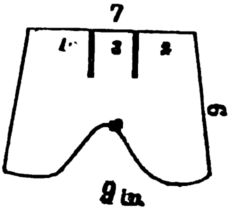
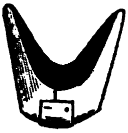


FIG. 12.



(This is about the average size for an adult.) Now fold No. 1 on No. 2 and No. 3 on top of these, and fasten with a pin, and you have the shape as seen in Fig. 12. This is to hold the splint while the plaster hardens. To make the splint, take a piece of muslin twice the size of Fig. 11, fold it on itself, and cut to the same size and dimensions as in the case of the paper-holder.

Wet this with water, open and spread out smooth, when the two extremities will be the widest. To six ounces water add enough dry plaster of Paris to make it as thick as cream, spread this immediately on the wet muslin, leaving a border of half an inch all round. Refold it again on itself, the plaster sides together; fold No. 1 to 2 and 3, as above described; place in the paper-holder, and apply to the jaw, supporting it with the hands for a few moments until the plaster sets. Remove, trim, if necessary, and dry, and it is ready for use. No time should be lost after the plaster is mixed in making the application.

This splint is usually made before the operation, and, when there is any external deformity, this can be readily put right by adding a little more plaster where it is needed. As all the plaster is held between the two folds of muslin, it will not soil the face. In case of fractures, this splint may be fastened to an interdental splint.

The splint may be retained in place by Dr. Garretson's bandage, applied in the following way: Take a strip of muslin, one and a half yard long and two inches wide. Place the chin on the middle of the bandage, carry up and cross on the forehead, then round the sides of the head to cross at the occiput, then forward and fasten at the chin.

Art. III.—*Impetigo Contagiosa; its Relations to Vaccinia.*

By Henry G. Piffard, M. D., Surgeon to the New York Dispensary for Diseases of the Skin.

In the last number of this Journal we gave a detailed history of a case of the so-called impetigo contagiosa of Fox, together with brief reference to another case met with shortly after, and also to a case which had come under the observation of Dr. Satterlee. I connection with these we announced the discovery, and gave the description, of a peculiar vegetable organism, the presence of which in all three of the cases afforded a reasonable explanation of the undisputed contagiousness of the affection. Since then we have been able to confirm the discovery by the examination of crusts from three additional cases, in all of which bodies identical with those described were found.

Before detailing the results of subsequent observation, we will give a brief exposition of the principal clinical features of the affection:

Impetigo contagiosa, according to Fox, frequently commences by the appearance of constitutional symptoms of a pyrexial character, varying in severity in different cases. In two or three days one or more small vesicles appear, followed from time to time by others. The vesicles gradually enlarge in size, and, two or three days later, dry into thin, light-yellowish or straw-colored scabs or crusts. The vesicles and crusts may be indefinite in number, and successive eruptions may prolong the disease for months. During the progress of the affection, associates, adults as well as children, may become the subjects of a similar eruption. Inoculation with the fluid contained in the vesicles will produce similar lesions, both upon those who are affected with the disease, and upon others, as has been experimentally determined by Fox and Taylor.

Left to itself, or improperly managed, the affection may last for an indefinite period, but, if properly treated, by the application of a mild mercurial, sulphur, iodic, or carbolic ointment, it may be promptly cured.

Dr. Fox, among other facts, states that the affection frequently follows vaccination, and refers particularly to this point in discussing the diagnostic features of the disease. In examining the several series of cases detailed by him,[1] we find that in almost every instance the first case of each series became affected shortly after vaccination. Our own experience, though much more limited, has been similar, and we will briefly relate it:

A child was vaccinated about two years ago by a friend and former pupil. During the second or third week thereafter a vesico-pustular eruption appeared upon it, and shortly after upon another child in the family. The parents, much alarmed, discharged the physician who had vaccinated the child, and carried the children to another practitioner, who sent them to me. The diagnosis of impetigo contagiosa was not made at the time; in fact, we expressed ourselves as unable to speak definitely concerning the nature of the affection. The cases were not eczema or ordinary impetigo, and we are now satisfied that they were examples of the affection under consideration.

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[1] *Journal of Cutaneous Medicine*, 1868.

A few months subsequently I vaccinated a young lady upon the calf of the right leg, with a crust taken from a perfectly healthy infant. During the third week after the vaccination she returned, and exhibited several vesicles and scabs of impetigo contagiosa upon the same leg and thigh. This case was cured in a few days, to the great delight of the young lady, and of her mother, who feared that some "unclean" disease had been inoculated with the vaccine.

A child, eight months of age, was vaccinated about the 1st of last October. During the latter part of the month an eruption of the same description appeared upon the arms and the upper part of the body. This case was presented to the New York Dermatological Society at its December meeting.

The two cases referred to in my previous article occurred shortly after vaccination, as did the case which came under the notice of Dr. Satterlee.

Through the kindness of Dr. Henry F. Walker, physician to the Nursery and Child's Hospital, I was enabled to see, on the 30th of May just passed, four children in one of the wards of that institution, upon whom there had recently appeared an eruption, presenting the features of impetigo contagiosa. The cases occurred in the following order:

J. H., about four months of age, had been vaccinated by the house-physician, some four or five weeks previous to the date of my visit. Two or three weeks later the eruption appeared, followed by a similar eruption upon H. S., aged four months, which was first noticed May 23d, followed by a similar eruption May 26th, upon J. D., aged thirteen months; and May 28th, upon C. G., aged one year.

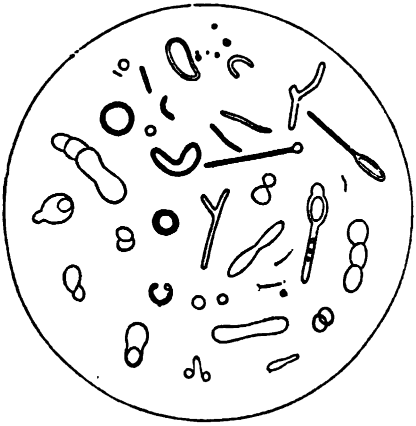
Crusts were taken from the first three, none having yet appeared upon the last, and, upon microscopical examination, exhibited the fungoid bodies referred to. Fluid from one of the vesicles upon the last case was examined by myself at the hospital, and found to contain nothing beyond some extremely minute molecular bodies, together with a few leucocytes, as has been observed by Fox.

The remarkable fact that impetigo contagiosa does undoubtedly, in a very large number of instances, develop itself as a sequela of vaccination, as exhibited in the recorded experience of Fox and others, as well as of myself, has led me for some time to suspect that there existed some closer connection between the two than mere coincidence. The coincidences were too frequent to be explained by the doctrine of chance. In fact, at the meeting of the Dermatological Society referred to, such connection was suggested by me, a view, however, which did not meet with favor. My subsequent experience tended to strengthen the conviction of the possibility of vaccinia standing in a causative relation to impetigo contagiosa, and induced me to make a careful microscopical examination of vaccine matter—for a plentiful supply of which I am under obligation to the vaccine establishment of the New York Dispensary, under the charge of Dr. F. P. Foster; to Dr. Darkin, of the Demilt Dispensary, and to their friends. Fresh lymph, in tubes from Dr. Foster, was carefully examined with amplifying powers of from 500 to 1,500 diameters. Nothing of an organized nature appeared upon examination, with the exception of minute bodies, by Beale termed "bioplasts," and by Chauveau "vaccinads." These bodies are supposed by both, and in fact pretty certainly determined by the latter gentleman, to be the active agents of vaccinia, and consequently a necessary and normal constituent of pure lymph. The result, as might have been anticipated, being negative, attention was directed to the examination of vaccine crusts.

Portions of vaccine crusts were placed in small phials and clean test-tubes, and a little more than covered with solutions of caustic soda, potassa, or ammonia, of varying strengths. After the lapse of from one to twenty-four hours, depending upon the strength of the caustic solution employed, more or less complete liquefaction of the crusts was found to have taken place. A drop of this fluid was removed from the phial, by pouring it upon the slide, and examined microscopically, with the same amplifying powers employed in examination of lymph. Upward of twenty vaccine crusts were examined, and in *every instance fungoid bodies, similar to those observed in impetigo contagiosa, were found*, with the sole difference that in vaccine crusts the fungus was more abundant and more luxuriant than in the majority of crusts derived from the other affection.

The vegetation appears under several forms (Fig. 1).

FIG. 1.



Fungoid Bodies found in Vaccine Crusts.

In the first place, there were found small, thread-like filaments, about the $\frac{1}{30,000}$ of an inch in breadth, but of varying lengths. These resembled the *Leptothrix buccalis* more closely than any other body with which we are acquainted. Secondly, there were small circles, ranging in diameter from the $\frac{1}{10,000}$ to the $\frac{1}{3,000}$ of an inch, and greatly resembling the red-blood disks except as to size, as has been before referred to. Thirdly, there were larger rings or circles; and fourthly, irregular figures of various forms. The circles, large and small, and the other figures, appeared as if they might have been formed by the junction of the ends of a filament, and exhibited every eccentricity of design which might be impressed upon an elastic ring. It is possible, however, that the irregular figures, instead of being formed from filaments, originally commenced as small molecules or rings, which, spreading peripherally, until they had obtained a certain size, had a different form impressed upon them, by forces the nature of which was not apparent. As before stated, change of form while under examination was observed on several occasions. This was particularly noticed in the instance of an extremely lengthened oval, the image of which had been projected upon the screen of a photographic camera by my friend Dr. J. W. S. Arnold. The oval form was gradually lost, and a circular one assumed, the motion being sufficiently rapid to prevent the reception of a sharp image by the sensitive plate. Photographs of some of the other forms, however, were successfully obtained by Dr. Arnold.

In some instances there were concentric rings, the circumferences of which maintained a uniform distance from each other, or were in absolute contact, as is shown in the shaded figures of the cut. Besides the above were found more or less cutaneous epithelium, epithelial *débris*, and fine molecular or granular matter, etc. No mycelium proper was observed, nor any indication of fructification. One figure in the cut bears a resemblance to spore-bearing mycelium; but such, we are satisfied, was not the case, the appearance shown being due to an accidental arrangement of the elements described.

The bodies we have described do not resemble those observed by Salisbury,[1] and termed by him *Ios vacciola*; nor any of the fungi obtained by Hallier[2] in his experimental cultivations of vaccine lymph; nor the fungus described by Kohn,[3] and met with by him in the examination of impetigo contagiosa. We have in two instances during these researches met with a fungus somewhat resembling the description given by Kohn, but, from the circumstances attending its appearance, we are inclined to regard its presence as accidental.

Nomenclature.—From the thin, hair-like character of the filaments, and the circumstances of their habitat, we suggest the name *Leptothrix vaccinalis* as an appropriate designation.

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[1] Salisbury, "Microscopical Examination of the Blood," etc., 1868.

[2] Hallier, "Parasitologische Untersuchungen," 1868.

[3] Kohn, in *Wiener Med. Presse*, June, 1871.

Practical Considerations.—Impetigo contagiosa, differing as it does in many important particulars from the eruptions with which we are most familiar, has undoubtedly in many instances occurring after vaccination been regarded as syphilitic, to the alarm of the patient, the annoyance of the vaccinator, and the chagrin of the physician who endeavors to remove it by internal medication. On the other hand, its general recognition as a parasitic affection, its trivial importance, and its ready curability by appropriate external applications, will enable us to calm the fears of the patient, to reassure the vaccinator, and to silence one more of the many objections which have been raised against the practice of vaccination by the prejudiced and unthinking.

ART. IV.—*On the Physiology of Syphilitic Infection, as applied to the Successive Manifestations of the Disease.*[1]

By FESSENDEN N. OTIS, M. D., Clinical Professor of Venereal Diseases in the College of Physicians and Surgeons, Surgeon in charge of the Genito-Urinary and Cutaneous Department of the Strangers' Hospital, New York.

ON a previous occasion I had the honor of reading a paper before this Society entitled "The Physiology of Syphilitic Infection." In this paper it was claimed that the infection took place through the agency of the lymphatic system *alone;* that it was the germinal matter of the tissue-fluids and of the blood—the unformed material—the white corpuscular element alone that was capable of entering into combination with the abnormal germinal cells in which the syphilitic influence was supposed to reside. These abnormal cells or disease-germs were assumed to be analogous to the disease-germs discovered by Beale and Chauveau in the blood of animals diseased by the cattle-plague, and in the fluid of the vaccine virus—germinal corpuscles, probably originating in the human organism, but possessing, besides the properties and powers of the white blood-corpuscle, a peculiar tenacity of existence, a capacity to grow and multiply when removed from the seat of their development and transferred to another situation, provided only that they were supplied with sufficient nutrient pabulum. It was further claimed that, through the current of the tissue-fluids which move constantly toward the lymphatic vessels, such disease-germs, deposited upon an abraded cutaneous surface, would necessarily be drawn toward, and might finally effect an entrance into, the underlying lymphatic vessels of the part. It was also shown that, as the influence of such disease-germs would be to induce a coagulation of the tissue-fluids, such coagulation would of necessity delay the transit of the disease-germ from the surface of inoculation to and into the lymphatic vessels, and that this delay favored the proliferation of the disease-germs; that the fibrinization of the tissue-lymph and the increased development of the connective-tissue cells at the point of inoculation would thus be effected to an extent sufficient to produce the induration which is looked upon as a characteristic of the initial lesion of syphilis. It was then claimed that the so-called *period of incubation* of syphilis corresponded to the time required for the disease-germ or its descendants to accomplish the transit from the inoculation-surface to and into the interior of the underlying lymphatic capillary; and that, after having effected an entrance into the lymphatic vessel, it passed directly along this canal until it was arrested by the lymphatic gland with which it was in connection.

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[1] Read before the County Medical Society, May 27, 1872.

Half a year subsequent to the publication of the foregoing views in the *New York Medical Gazette*, and in the *American Journal of Syphilography and Dermatology* (July, 1871), it was announced at a meeting of the Medical Society of Vienna, January, 1872, that Dr. Lostorfer, of that city, after researches and experiments in the laboratory of Prof. Stricker, and in the syphilitic wards of Prof. Zeissl, covering a period of several months, had finally discovered and demonstrated the existence of the living syphilitic corpuscle or disease-germ, and its presence in the blood of persons affected with syphilis. This corpuscle was described as living bioplast, growing and multiplying after having been removed from the seat of its development; in short, corresponding most completely with the disease-germ which I had assumed as the infecting agent in syphilis. Out of about fifty specimens of healthy and diseased blood, marked and numbered, and presented to Dr. Lostorfer by Profs. Hebra and Stricker, *twenty* were selected as containing the syphilitic corpuscle, and, by reference to marks and numbers, in every instance it was found that the specimens had been taken from persons affected with syphilitic disease. The conditions under which this corpuscle was found, and the manner of its development, were so in harmony with the probabilities of such a discovery, that, with the corroboration thus afforded by Dr. Lostorfer, the syphilitic corpuscle was publicly accepted and confirmed by Profs. Skoda, Stricker, and Hebra. It was, however, soon after disputed by Profs. Wedl, Gruber, and Neumann, although no especial investigation of the subject appears to have been made by those gentlemen. Prof. Stricker, however, at once commenced a series of independent observations, for the purpose of ascertaining the validity and real significance of Dr. Lostorfer's discovery. Advices received from Prof. Bumstead, by the last steamer, are to the effect that the corpuscle described by Dr. Lostorfer was actually found by him in syphilitic blood, but it was also found to have been present in the blood of persons not syphilitic, but who were the subject of cachexia from other causes. He then concludes that the corpuscle of Lostorfer is a product of the cachectic condition from any cause. The significance of this cachexia-corpuscle, when present, has not yet been ascertained.

Had the discovery of Dr. Lostorfer's corpuscle and its exclusive association with syphilis been confirmed, it must have been accepted as a corroboration of the views I have advanced. It has, however, proved otherwise, and yet I claim as confidently as before that the necessity for such a corpuscle still exists, and that a present failure to demonstrate its actual presence in the blood of syphilitics does not militate against its acceptance as a reasonable and probable initiative point in the attempted elucidation of the acknowledged mysteries of syphilitic disease.

I propose to-night to resume the subject of syphilitic infection at the point which is recognized as the termination of the first incubation of syphilis, and to consider the disease-germ, in its passage through the lymphatic glands, adjacent to the surface of inoculation, in its relation to a second period of rest which is known to elapse between the induration of the initial lesion and the earliest period at which the constitutional manifestation of the disease ever takes place—a period termed, by some authors, the *second incubation* of syphilis.

It is a universally-accepted clinical fact in the natural history of syphilis that a distinct period of rest succeeds the appearance of the initial lesion or chancre; that, for a time, the chancre is the only recognizable evidence of syphilis in the individual bearing it. That by no external sign or internal sensation is it possible to discover any variation in the normal physiological processes of such subject that can be attributed to the influence of syphilitic disease. It is, however, claimed, and by a very great weight of authority, notwithstanding the complete absence of proof, that the development of the disease is steadily and surely progressing in the blood, and that it will finally acquire and exercise the power of producing general manifestations of the disease; and hence it is claimed that this period of apparent rest is an incubation as actual and complete as that which is known to elapse between the date of vicious contact and the appearance of the initial lesion or chancre.

According to authorities, the earliest and most common general manifestation of syphilis is a *roseolous eruption.*

Accepting the occurrence of this exanthema as the termination of the second period of incubation, Ricord places its accession at three weeks from the appearance of the chancre as the earliest, and at six months as the latest date at which he has ever known it to occur. Cullerier makes a similar statement. Lancereaux places it at from six weeks to two months, Sigmund from four to six weeks, Henry Lee from four to eight. Zeissl has never seen this eruption before the eighth week after contagion. Berkeley Hill confirms this experience, and further states that the longest interval he had observed was fifteen weeks. Diday cites fifty-two cases carefully observed, where the appearance of the chancre was ascertained, and where no treatment was resorted to, where the interval varied from twenty-five to a hundred and five days. Fournier, from an examination of three hundred and seven cases, concludes that this eruption, "the earliest general manifestation of the disease," appears from the fortieth to the fiftieth day after the development of the chancre. Puche and Cullerier the younger state that roseola is never wholly absent in syphilis, and concur in the previous statement as to the date of its appearance. Bumstead regards the occurrence of the syphilitic roseola between the fortieth and fiftieth day after the development of the chancre as almost certain, and states that he has never seen a case, carefully observed, where it had failed to show itself within three months.

Thus it appears that, with great unanimity, authorities agree upon a period of rest, following the appearance of the initial lesion or chancre, of at least three or four weeks. Absent in none, it is shown to have been prolonged in exceptional cases to three or four months. Virchow, in his "Cellular Pathology," p. 221, has furnished a parallel to this apparent cessation of progress in the early stages of syphilis, by showing what is known to occur in cases of mammary cancer, where, during a long period, only the axillary gland becomes implicated, the groups of glands next in succession, as well as all other organs, remaining free from disease. "We can account for this," he remarks, "upon no other supposition than that the gland collects the hurtful ingredients absorbed from the breast, and thereby, for a time, affords protection to the body; but at length proves insufficient, nay, perhaps, at a later period, itself becomes a new source of independent infection to the body, inasmuch as a further propagation of the poisonous matter may take place from the diseased parts of the gland." I have, however, yet to find this view of Virchow's accepted, by any of the numerous writers on syphilis, as applicable to the interval of rest following the appearance of the initial lesion. The explanation of this remarkable phenomenon, given and generally accepted, is, that the disease has an inherent cyclical tendency—proceeding throughout its course by successive stages or bursts; that, notwithstanding the apparent rest, the vitiation of the circulatory fluids is going steadily, relentlessly on; and that, when the system becomes fully charged, an outbreak takes place, and is recognized in the syphilitic roseola. Thus a mystery, wholly incompatible with known physiological and pathological facts, is quietly accepted, almost without comment, and designated *the second period of incubation.*

A certain and positive value must attach to a theory which shall even plausibly account for and explain a mystery. A *point d'appui* is thus fixed, about which the results of observation and investigation may crystallize—it is a step out of the *unknown* toward exact science.

In the case of the first incubation of syphilis, clinical cases were adduced, supporting the theory of a gradual local development of the influence of the disease-germ, up to the point of its final entrance into a lymphatic vessel. It was then shown that, through natural physiological processes, the disease-germ or its descendants would be carried along the lumen of the lymphatic canal until it reached the lymphatic gland in connection with that vessel, and that the arrest of the vitiated corpuscle in the substance of the gland was in accordance with the previously ascertained influence of the so-called syphilitic virus, viz., to induce a coagulation of the tissue-fluids with which it was brought in contact.

The anatomical arrangement of the lymphatic glands has long been a point of inquiry. By early observers considered as consisting simply of coils of lymphatic vessels, they were demonstrated by Kölliker to possess a parenchyma in which the afferent vessels were wholly lost, and out of which the efferent vessels constituted themselves at the opposite side of the gland. This view, confirmed by Virchow, was generally accepted by histologists until within the last two years, since when, through the investigations of His, Frey, and Teichmann, and substantiated by Recklinghausen, it has been shown, that the circulation through the glands is continuous by the presence of lymph-paths lined with epithelium throughout the supposed parenchyma, and that consequently a continuous lymph-current through the substance of the glands could be demonstrated.

Accepting this view, we have, then, as in the application of the disease-germ to the original abraded surface of a syphilitic inoculation, the same two distinct forces by which the progress of the vitiated corpuscle through the tissues of the lymphatic gland would be facilitated, viz., the independent power of amœboid movement of the corpuscle, and the force of the lymph-current through the substance of the gland, delayed in its progress by the same cause which produced the interval of apparent rest occurring between the application of the virus to the surface of inoculation, and its arrival at the portals of the lymphatic gland of connection, corresponding to the period of first incubation. The coagulation of the lymph, effected by the presence of the disease-germ, may be accepted as producing a similar retardation in its passage through the substance of the lymphatic gland in connection, and thus reasonably explain the period of rest known as the second incubation of syphilis.

As a result of the coagulating influence of the disease-germs or their descendants, in the lymph-paths traversing the lymphatic glands, we have, then, an arrest more or less complete of the lymph-current. The natural result of this would be an increased proliferation of the lymph-corpuscles both normal and vitiated, and their rapid accumulation within the gland; and we should expect to find following such occurrences an immediate enlargement of the substance of the glands. This is exactly what we *do* find in every case: a development, a condensation of the normal constituents of the gland, a simple hyperplasia, which is always sluggish and non-inflammatory, and is recognized as characteristic of syphilis. We must also have, to a greater or less extent, and from the same causes, the same retardation of the vitiated corpuscles on their passage through the lymph-vessels and glands, which still interpose between the glands of original implication and the thoracic duct, through which they finally effect their entrance into the general circulation. As an evidence that this is the course and influence of the disease-germs, after their passing through the first order of glands, we have a sudden increase in size of the glands of the second order—glands remote from the point of inoculation, the cervical, the submaxillary, the epitrochlear, etc., the slight but well-marked increase of temperature, a marked increase of the white corpuscular element of the blood, and various other evidences that the constitution at large is in embarrassment and distress in the performance of its normal nutritive and nervous functions. Prominent among these stands the roseolous eruption of this period, which is claimed to mark the termination of the second period of rest or incubation, and to usher in the active secondary or constitutional stage of syphilis.

The period of so-called second incubation, then, whether caused by a necessity in the nature of the syphilitic virus, requiring a certain accumulation in the blood before general constitutional disturbances can take place, or caused by the gradual passage of the vitiated germinal matter through the glands and vessels of the lymphatic system, intervening between the point of inoculation and the general circulation, is nevertheless an admitted fact, and the roseola is accepted as marking its termination.

Let us inquire into the significance of this roseola. Is it really the result of pathological changes, necessarily dependent upon the syphilitic influence? Or may we not be able to class it among the physiological phenomena, and thus show that it is not due to an elimination or an attempted elimination of syphilitic material from the infected organism.

Mr. Wilson, the distinguished English dermatologist and syphilographer, gives the following description of this exanthem: "Roseola syphilitica commonly presents itself in the form of undefined patches, giving to the skin an appearance identical with that of common idiopathic roseola or measles, and is due, like the two latter, to the manner of distribution of the blood-vessels of the skin. Perhaps," he says, "the congested patch represents the ramifications of a single arterial trunk; perhaps it embraces that small capillary system which is normally emptied by a separate venous trunk; perhaps, again, it includes the small district of skin, the circulation of which may be governed by the ultimate divisions of one small nervous twig."[1]

With this evident uncertainty as to the manner in which simple roseolas are produced, he does not hesitate to express a decided opinion that the syphilitic roseola is caused by an effort of Nature to expel the syphilitic poison from the system, and compares the syphilitic disease to the simple exanthemata. "It is preceded, he says, "by a peculiar fever called the syphilitic, and bears a resemblance in its pathognomonic symptoms to measles, scarlet fever, and small-pox, as follows: 1. The nervous depression, showing the stagnating influence of the accumulated poison. 2. In the congestion of the mucous membranes, particularly that of the fauces, showing the effort made by the blood-vessels to eject the poison through that tissue; and 3. In the cutaneous exanthema which *complete the triumph of the pressure from within, and is the sign that the poison is driven to the surface, and is in process of expulsion.*" There is, then, no uncertainty about the fact that Mr. Wilson considers syphilitic roseola the result of the process of the elimination of the syphilitic material through the skin. This view, elaborated more fully by Mr. Wilson than by any other writer on cutaneous or syphilitic disease, I do not find contradicted by any except M. Diday, of Lyons, but is accepted directly or through implication by all the other authorities I have been able to consult. Very naturally, Mr. Wilson does not stop at this point, but goes on to trace a connection between syphilitic roseola and the subsequent manifestations of the disease. Thus, in his last "Lectures on Dermatology" (published London, 1871), he says: "Chancre, the focus of inlet of the poison, has run its course and healed up. Six weeks afterward, an exanthema, which is identical in appearance with roseola, is developed in the skin. . . . Another six weeks passes by, and a second exanthema appears, no longer a roseola like the first, but an eruption of *papulæ*, corresponding to the common *lichen.* Then another interval of six weeks transpires, and the eruption may appear for the third time, but with dimensions still further augmented, viz., as a large papula or tubercle." In an earlier work on syphilis, he says: "The transition of *syphilitic roseola* into *lichen* is so obvious that it may be observed through every stage of its progress. A roseolous patch may be seen to develop papules . . . and I have furthermore seen the small papules of lichen converted into the larger elevation called tubercle, by a sudden aggravation of the syphilitic fever, or from exposure to cold." Of the *simple roseola*, however, he says in his last published lectures, 1871, "it is now well understoodthat the influence of the *vaso-motor nerves* is involved in the production of roseolas." But for the syphilitic roseola, Mr. Wilson has assumed a pathological importance which could not attach to it, were it shown to be possibly due to the impression made upon the sympathetic nervous system by the syphilitic influence.

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[1] "Diseases of the Skin, a System of Cutaneous Medicine," by Erasmus Wilson, F. R. S. Sixth edition, p. 472. London, 1866.

Characteristic prodromata are recognized by modern syphilographers as constantly associated with the advent of constitutional syphilis in representative cases. They are numerous and varied, as follows, viz., slight persistent increase of temperature, 99° to 101°, general *malaise*, headache, rheumatoid pains, irregular, chiefly nocturnal, loss of appetite, indigestion, nausea, diarrhœa, giddiness, mental irritability, sleeplessness, great prostration, unnatural acceleration of breathing on slight exercise, the circulation generally disturbed, in some cases exacerbations of fever of an intermittent character, temperature running up to 103° or 104°, followed by profuse sweats, etc. These are among the most prominent of the prodromata cited by authors, and are grouped together under the title of *the syphilitic fever*. This title is then applied not simply to an acceleration of the pulse, heat of skin, etc., which is usually understood by the term fever, but any one of the foregoing conditions or symptoms, or any modification or combination of them, is claimed to merit this title. Diday uses the more exact and appropriate term *syphilitic prodromes*. It will then be readily seen that, when authors speak of *syphilitic fever* as ushering in, or being associated with, syphilitic roseola, a very considerable degree of uncertainty may be claimed as to the dependence of such varied and common aberrations from the healthy state upon the development of syphilitic roseola.

The connection assumed by Mr. Wilson between syphilitic fever and syphilitic roseola is, however, not universally accepted. His statement that the syphilitic roseola is preceded (ushered in) by fever, especially with the broad interpretation previously given to the term, would lead us naturally to infer that this was the rule. Ricord, on the contrary, says that it is *never* associated with fever, except as an accidental coincidence; and that, when fever is present, it is invariably due to some cause *quite independent of the roseola.* Bassereau affirms that it was entirely absent in fifty-six out of the one hundred and ninety-nine cases critically observed by him, but it occurred in thirty-four out of fifty cases of the later papular eruption. Coulson states that one-third of the cases of syphilitic roseola are not preceded by any premonitory symptoms. McCarthy, quoted by Lancereaux, that of sixty cases only forty were preceded by any well-marked prodromes. Of these forty, twenty-one had nocturnal cephalalgia alone, eleven cephalalgia and rheumatoid pains, and eight the *latter* alone; leaving twenty cases, or *one-third*, occurring absolutely without any premonition of any sort. Bumstead says that syphilitic roseola is so free from febrile excitement, heat, and pruritus, that the patient may not discover its presence except by accident.

Besides the records of observations covering a period of nearly two years in the Blackwell's Island Venereal Hospital of this city, in 1851 and 1852, and, during the subsequent years, meeting frequent cases in private practice, I have called especial attention to this matter in every case presenting at my clinique in the College of Physicians and Surgeons of this city for the past two years, and for the last year, in the venereal wards of the Strangers' Hospital under my charge, and I do not hesitate now to express my entire accordance with the views of M. Ricord, viz., that the syphilitic roseola is *never* preceded by *nor associated with* any fever or other prodromata that cannot readily and reasonably be accounted for through causes entirely independent of the approach or the presence of the roseola. For the past twenty years, Mr. Wilson has taught, without qualification and almost without contradiction, that syphilis, with roseola as its early exponent, was a distinct element; that the organism in process of infection was gradually invaded by it, until Nature, no longer able to endure its presence, concentrated all its forces at a given stage to expel it with the directness and urgency of a woman in labor. As he puts it in his climacteric in describing the roseola, "completes the triumph of the pressure from within, and is the sign that the poison is driven to the surface, and is in process of expulsion."

In the effort to understand the mysterious workings of syphilis in the human organism, analogical reasoning has naturally been resorted to; and from the fact that there occurs a well-ascertained period of rest in its early development; that it is associated with varied disturbances of the nervous and vascular systems; and that preceding, during, or succeeding these disturbances, an exanthematous eruption occurs, it has been the fashion, for the want of a better place, to class it among the exanthemata, and further, inasmuch as it has been the accepted conclusion that these diseases are the result of an effort of Nature to rid itself of the peccant material causing them, a like interpretation has been seized upon to explain the significance and value of the syphilitic roseola. The very important fact that, at the point of commencing constitutional disturbance in syphilis, *the entire lymphatic, vascular, and glandular systems are the subject of recognized and grave interference*, seems to have been entirely lost sight of. There is nothing better understood than that, among the first constitutional evidences of the syphilitic influence, an engorgement and enlargement of numerous lymphatic glands remote from the point of inoculation take place, and that there is a marked proportional increase of the white corpuscular element of the blood coincident with this glandular enlargement. Has this undoubted disturbance in a system of important functional use, one now recognized as occupying a place of acknowledged value among the hæmatopoëtic organs, no influence in producing the *prodromes* of constitutional syphilis? Or must this be ignored, and all aberrations from the healthy condition be attributed to the influence of a simple roseola which has been shown to be present and associated with recognized deviations from the healthy state in but a very limited proportion of cases?

Diday states it as his opinion that, in the prodromes of syphilis, mercurials are contraindicated, and that iron and iodine will be found the best means of combating them. In this view, Lancereaux explicitly concurs, although he deprecates the natural inference to be drawn from such concurrence. "But are we," he[1] says, "like Diday, to conclude, from the fact that iron and iodine are the means to be employed for combating syphilitic fever, that this morbid condition has not the same relation to syphilis as the constitutional lesions? that it is *occasioned*, and not *directly* caused, by that disease? Not absolutely," he answers, "for, if it be permitted to attribute to the debilitation, impressed upon the organism by the syphilitic poison, *some* of the disturbances in question, a great number remain which do not explain either the chlorosis or the anæmia." Forced practically to acknowledge the functional disturbances as in no immediate relation to the syphilitic virus, he is still unwilling to admit that the direct action of that virus is not in some mysterious way the local cause of certain of the prodromes, viz., those which cannot be made to connect with the immediate and perceptible blood-changes. That these changes, well-recognized departures from the healthy state of the blood, do occur, is no longer a question, but there is a considerable variance of opinion as to the *cause* of the disproportion of the normal constituents of the blood which obtains in the early stage of syphilis. Virchow claims that it is a leucocythemia. "In any considerable irritation of a lymphatic gland," he[2] remarks, "there is an increased formation of cells in it, and this is followed by an increase in the proportion of lymph-corpuscles in the blood. Billroth,[3] on the contrary, thinks it unlikely that the hyperplasic lymphatic glands should be the source of an increased supply.

"1. Because extensive adenopathies occur independent of such increase.

"2. That it is very improbable that, when the normal channels of the glands are obstructed, they should continue to perform their physiological functions." In the attempts made by Frey, O. Weber, and Billroth, to inject the vessels of hyperplasic lymphatic glands, there was failure in every instance, thus affording negative proof that the enlarged glands were physiologically insufficient. Rindfleisch,[4] however, distinctly states that lymphatic glands, the subject of aggravated hyperplasia, are still readily injected. "One is able," he says, "to fill the lymph-sinus of such glands, as well by the afferent vessels, as by piercing them."

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[1] Lancereaux, vol. i., p. 181.

[2] Virchow, p. 222.

[3] Billroth, "Surgical Pathology," p. 209.

[4] Rindfleisch, "Text-book of Pathological Histology," p. 186, 1870.

In reports of the experiments made by Grassi, under the direction of M. Ricord, the disproportion found existing between the white and the red corpuscles was claimed to have been caused by a *diminution* of the *red* corpuscles.[1] It is now known that the red blood-corpuscles are regenerated at the expense of the white; and, with this in view, Mr. T. Henry Green, of London, suggests that "conditions obtained when the white corpuscle already in the blood is arrested in its development into the red, and that possibly the white corpuscles increase by multiplication in the blood."[2] With this present disagreement of authorities as to the cause and absolute value of the leucocythemia, or chloro-anæmia, always associated with the real prodromes of syphilis, it may still be accepted as exercising a profound influence upon the nutritive and nervous functions during the period of its presence, which, curiously enough, coincides and passes away with the period of the prodromes of syphilis.

In his elaborate and exhaustive work on diseases of the skin, Mr. Wilson treats of eighteen different varieties of roseola. Of these, six, viz., roseola rheumatica, roseola vaccinia, roseola cholerica, roseola arthritica, and roseola febris, continuæ, occurring exceptionally in the course of the corresponding diseases, he calls the *symptomatic group* of roseolas. "The cause," he says, "may be *nutritive*, *nervous*, or *assimilative*." He has already told us that the *vaso-motor nerves* are concerned in the production of roseolas, that is to say, *all* roseolas *except* the roseola which occurs during the early evolution of syphilitic disease. *This* is claimed to be the result of a pathological condition, and to be but the first stage of the later eruptions, papular, tubercular, etc. That the papular eruption may be developed at a later period, and upon the exact spots previously occupied by the roseola, and that these in turn, passing away, may be replaced by a still later *tubercular* eruption, is not only possible but almost inevitable, where they occur at all; but how are we to understand this gradual development when we find both the papular and roseolous eruptions associated together at the same time, each distinct and characteristic, and absolutely independent of the other? This fact, which I have often seen, and have demonstrated in my clinique on numerous occasions, will, I doubt not, be confirmed by every critical observer of the early eruptions of syphilis.

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[1] Stricker, "Human and Comparative Histology," Sydenham Society's edition, vol. i., p. 421.

[2] "Pathology and Morbid Anatomy," Green, London, 1871, p. 132.

Virchow[1] states that, on a section of the sympathetic nerve in the neck of an animal, "a state of hyperæmia ensues in the whole of that half of the head. The ears become dark red, the vessels greatly dilated, the conjunctiva and nasal mucous membrane turgidly injected; and this," he says, "may continue for days, or weeks or months without the least appreciable nutritive disturbance necessarily arising thereupon." From deductions based upon the electric experiments of Claude Bernard upon a bisected sympathetic, he states that, "whether the relaxation of the muscular fibres of a vessel be produced directly by a paralysis of the nerve, or by an interruption of the nervous influence, or whether it be the indirect result of a previous stimulation, giving rise to exhaustion, in *every case*, we have to deal with a *kind of paralysis* of the walls of the vessel, and that the process is incorrectly designated *active hyperæmia*, inasmuch as the condition of the vessels in it is always a *completely passive* one."

Belladonna is well known as a relaxing agent in its effect upon the iris, and also in producing an eruption similar to the roseola of syphilis.

Mental emotions are recognized as the cause of eruptions in not dissimilar cases. I have at present under observation a lady upon whom an eruption, in no way to be distinguished from a classical recent syphilitic roseola, is brought out over the breast, back, and arms, whenever she is subjected to any unusual mental excitement, and which remains distinct for several hours, as we know that similar roseolas are initiated by causes that can only be referred to impressions made upon the sympathetic nervous system.

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[1] "Cellular Pathology," Virchow, p. 156.

But, it may be remarked, while certain of the syphilitic roseolas pass away in a few days, or even hours, yet, in a very considerable proportion, the eruption is much more persistent than in the simple roseolas, and besides, in finally passing off, they leave a coppery stain which is considered characteristic of syphilis.

To this it may be answered that the causes of simple roseolas are transitory, and yet *a thought*, in the lady above referred to, caused a rash which remained for several hours. Is it, then, remarkable that a cause dependent upon actual changes in the blood proportions and quality should be more persistent? It is certainly *the fact*, and to this very persistence, and to it alone, are due the pigmentary sequelæ which are thought to be so significant. From the complete and prolonged stasis of the blood in the relaxed capillaries, the *hæmaglobin* (the coloring-matter of the blood) escapes through the vascular walls to some slight extent, and into the superimposed tissue; after a time crystallization of the *hæmaglobin* takes place, forming *hæmatoïdin*, the reddish-yellow crystals of which occasion the coppery stains of the skin—a result which may occur not alone in syphilitic roseola, but in any prolonged blood capillary engorgements from any cause. May we not, then, reasonably infer, from the foregoing, and in the absolute absence of proof to the contrary, that the syphilitic roseola is, like the simple roseolas, the result of an impression upon the sympathetic nervous system—a paresis of the vaso-motor nerves of the cutaneous envelope, caused by a limited but special paralyzing influence exerted upon the great sympathetic nerve through occult but positively recognized blood-changes?

We have now followed the progress of the syphilitic disease-germ, from its original point of contact with the surface of inoculation, to its lodgment in the second order of lymphatic glands. If the foregoing view of the nature of the roseola of syphilis be accepted, we have *no positive evidence that the disease has yet reached the blood.* Delayed for a limited period at the point of inoculation by unavoidable physical and physiological obstructions, it finally reaches the first order of glands (the glands of connection); delayed here again for another period by the same causes that obtained in the first instance, it finally passes on through the afferent lymphatic canals to and into the second order of glands, which become involved in precisely the same manner as the first. Thus, if we are willing to accept the view of Diday, that the prodromes of syphilis are due to the chloro-anæmia, the sole evidence of the presence of the so-called *syphilitic* virus is still to be found in the lymphatic system. We have, it is true, at about this time, in a considerable proportion of cases, an inflammatory engorgement, more or less intense, occupying the mucous membranes of the pharynx and tonsils; but the tonsils have been recently accepted as belonging to the lymphatic system, representing a simple form of lymphatic gland, although no direct communication has yet been demonstrated between the follicles composing the tonsil and the adjacent lymphatic vessels; these cover them in almost completely, and they are besides so dilated that they are rather lymph-sacs or lacunæ surrounding the follicle.

The entire pharynx is much richer in lymphatics than the surrounding mucous membrane. At this point, then, we should expect, with the general engorgement of the lymphatic system, early evidence of disturbance. Brought here into more intimate relations with the blood vascular system than occurs in lymph-glands of a higher order, the implication of these lymphatic accessories is often followed by a true inflammatory engorgement, which not unfrequently results in a rapid gangrenous or ulcerative destruction of tissue.

In the necessarily frequent allusion to different authorities, as the subject of syphilis is considered, the term *virus* constantly occurs; it is, therefore, perhaps not out of place here to repeat that, according to the views advanced in this and in the preceding paper, it is not a *virus* which is accepted as producing syphilis, but an abnormally active natural germinal cell or corpuscle, that, being introduced into a healthy organism, impresses the normal germinal molecules with an action similar to its own; that the nutritive changes and deviations from health in syphilis are simply the result of a too rapid development of such germinal material as the hyperactive corpuscle may have been able to impress with its influence; that in no case is the disease-germ or its descendants able to arrive at any maturity; that they never become elevated into red blood-corpuscles, nor ever form a permanent ingredient in any useful tissue; that, as certain conditions of vascular excitement, which we call inflammatory, hasten the development of the white blood-corpuscle, up to the point of emasculation, resulting in the pus-corpuscle, so a more subtle influence imparts to certain of the white blood-cells a greater tenacity of life and a capacity of somewhat higher development, capable of a certain low power of organization, but still falling short of the ability to combine with the normal tissues, in any way, except as foreign material, to embarrass and disturb their normal development and functional integrity.

The vitiated corpuscles, then, holding on their course through the lymphatic system in which we have observed them repeatedly, and for a time effectively, arrested by glandular barriers, finally gain an entrance into the general circulation.

It is by no means improbable that the lymphatic glands act as depots, as claimed by Virchow, and that, in occasional cases, the vitiated corpuscles are retained in these organs, until a normal or artificially excited fatty degeneration has enabled them to be partially or wholly eliminated from the system. In this way, the occurrence of cases of syphilis presenting marked deviations from the usual course of the disease may be accounted for. In classical cases, however, there seems to be no reason to doubt but that the vitiated germinal material passes into the blood along with the normal lymphatic elements.

The earliest evidence that this occurrence has taken place appears in the very great majority of cases in the development of a papular eruption—symmetrical and more or less general—and usually preceded by or ushered in with an increase of temperature, an acceleration of the pulse, and variable degrees of nervous and functional disturbance. This eruption is usually composed more or less from distinct elevations of the skin, varying in different cases from the size of a pin's-head to half a dime, or even larger. Mr. Wilson, consistently with his teaching in regard to the syphilitic roseola, remarks that "when the eruptive force is sufficiently powerful, the case is one of lichen.[1] In this form there sometimes appears a tendency to aggregation into small groups or clusters; this is known as the *lichen corymbosus.* Other varieties present, dependent upon size or arrangement, as the lenticular, the papular, the annular, as described by different authors. The seat of origin of these papular eruptions appears to be still in dispute. Zeissl thinks that the sebaceous or the hair-follicles are the principal seat. Mr. Wilson, in describing a case of the lichenoid form observed by him, says: "The aperture of a follicle was apparent at the summit of each papule, marking the seat of the inflammatory congestion to be the capillary plexus [of the follicle." Lancereaux believes that the papillæ of the dermis are also sometimes the starting-point of this affection. Rindfleisch says of the simple papulous exanthemata that "the exudation is seated in the papilla itself." According to Mr. Tilbury Fox, "the true papule is due to a 'plastic exudation' into the skin, especially of the papillary layer." Possibly it can be shown that the origin of the papule is *always*, and of *necessity*, in the *papillæ cutis.*

"The blood," says Rindfleisch, "is the medium of exchange of the material of the organism. It is the nutritive fluid which conveys to each individual portion of the body the nutritive ingredients necessary for its existence, and instead, carries away from the parts the useless and injurious products of the chemical processes associated with nutrition."[2] This circulating medium is carried out, from the centre of the circulation to the periphery, by the arteries and returned by the veins. Intercalated between these two sets of vessels is the lymphatic system, which withdraws from the tissues such nutritive materials as are exuded in excess by the arteries into the tissues, and returns them again into the circulation at a given point. Now at the nearest points of contact of these systems of vessels, at the superficies of the body, we should expect to find collections of surplus material, which, after having escaped from the arteries, were unable from any cause to effect an entrance into the under-drainage vessels, the lymphatics. The relation of these vessels in the cutaneous envelop, as described by Teichman, is as follows: "The blood-vessels lie around the circumference of the papillæ[1] (winding up, corkscrew fashion, around the papilla until they unite at its apex); while the lymphatic vessels lie exactly in the centre of the papillæ, and no lymphatics are found in the sebaceous follicles, the sweat-glands, or the hair-bulbs. Consequently, it is in this juxtaposition of the blood-capillaries surrounding the papillæ, and the lymphatic capillaries passing up through their centres, that is to say, in the interval between these, that we should expect to find materials escaped from the blood-capillaries, detained in their transit into the lymphatic capillaries. At this point, the force of the circulation is at the minimum, the condition most favorable for proliferation. The act of proliferation is a vital one, one predisposing to the coagulation of the fibrine contained in the tissue-fluid.

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[1] Wilson on "Diseases of the Skin," 1866, p. 474.

[2] Rindfleisch, p. 181.

With these predisposing forces and conditions, it is natural to expect an accumulation of cell-material and a separation of the fibrine of the lymph, and these resulting in a hyperplasia of the papillary layer of the cutis. This, too, most prominent, in the interval between the blood-capillaries distributed around the summit of the papilla and the lymph-capillary which passes up through its centre, necessarily giving rise to the abnormal elevations termed syphilitic papules. These papules vary in size, as the hyperplasic papillæ are more or less abundant—appearing in lichen as quite discrete, while in the larger varieties an aggregation of papillæ occurs, and is consolidated through the escape of the hyperplasic materials into the adjacent surrounding tissue. We are thus enabled to account for the variations in the size of papules, occurring as a result of the proliferation of cell-elements in the papillary layer of the cutis.

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[1] Rindfleisch, p. 277.

The recent observations of Kohn, 1870,[1] in regard to the histological constituents concerned in the formation of the papular syphilides, confirm the foregoing views: "They are formed," he says, "uniform and dense, limited clearly by infiltrations into the papillæ and corium. These infiltrations are constituted by an accumulation of cells, which are piled up in dense and regular layer, around the vessels in the interstices of the connective tissue. The cells are not destined to be permanently organized, as they degenerate and disappear, or assume a dull granular appearance, undergo fatty degeneration and are absorbed, or they may become heaped together in the form of detritus and form pus. Upon the section of a papule, it is seen that it is limited by two lines formed of cells which are accumulated in the corium and on a level with the papillæ. The latter two structures are glued together, while the epidermis is stretched. The papule is resistant, in consequence of the accumulation of the cell-elements, and its color is due to the capillary stasis, to any effusion of the coloring-matter, and perhaps to the color of the new formation." These observations of Dr. Kohn are in complete accord with the previous statements made by Auspitz,[2] Virchow,[3] Neumann,[4] and Taylor.[5]

The syphilitic papule, then, whether occurring as a local or a general eruption, may be considered as the result of a proliferation of the cell-element, and an organization of the fibrine, separated from the tissue-fluid, and which has usually, if not always, a papilla cutis as its seat of origin. Here we find the point of nearest circumscribed contact or closest relation, viz., between the blood-capillaries and the intercalated lymph-vessel of the papilla, and *not* in the sebaceous follicle, or in the hair-bulbs, or in the sweat-glands, with which lymphatic vessels have not been found to be so associated. In regard, therefore, to these latter, we are led to conclude that when they are apparently engaged in the formation of a syphilitic papule, it is only by their being aggregated into the papule by the exudation of cell-material which has originated within the papilla cutis. It is not now, perhaps, too much to assert that the syphilitic papule is always an evidence of an abnormal proliferation, excited by the influence of the syphilitic disease-germ, or corpuscle which has escaped from a blood capillary in a papilla cutis, and occurs separately or in groups, the arrangement differing in different cases, from influences not yet determined.

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[1] "Caractères cliniques et histologiques des Syphilides," par Moritz Kohn. Wiener Med. Wochenschrift, 1870, No. 55. Archives Générales de Médecine, March, 1872.

[2] Auspitz, "Ueber die Zellen Infiltration der Leiderhaut," Medizinische Jahrbücher, vol. ii., p. 208, 1864.

[3] Virchow, "Pathologie des Tumeurs." Trad. Franc., vol. ii., p. 361, *et seq.*

[4] Neumann, "Lehrbuch der Hautkrankheiten," p. 240.

[5] Taylor, "Observations on the Papular Syphilides." *American Journal of Syphilography and Dermatology*, April, 1870, p. 108.

We have, besides the well-marked papular syphilide, pustular and vesicular eruptions associated with the early period of syphilis, and from the manner in which the papular syphilide has been claimed to originate, it is easy to understand why the pustular and vesicular forms of eruption, occurring at a similar period in the progress of the disease, often mingled with and visibly originating from the syphilitic papule, should be considered simply as abortive papular syphilides. Kohn, it will be remembered, remarks of the syphilitic papule, that "the cells composing it are not destined to be permanently organized, as they degenerate and disappear, undergo fatty degeneration, and are absorbed, or are heaped together as detritus, and form pus."

The pustular and vesicular syphilitic eruptions, then, would seem to result from a lack of formative power in the lymph, and from the inability to form, or the easy liquefaction of the hyperplasic materials, this occurring probably from a low state of the system, produced either by general causes or by some especial dyscrasia, not necessarily dependent upon the syphilitic influence.

Prominent among the manifestations of early constitutional syphilis is the mucous patch. This consists of an erosion of the surface or a complete destruction of mucous membrane upon which this peculiar lesion is always situated. The erosion or entire disintegration of the mucous membrane is followed by the exudation of lymph from the denuded surface and its rapid organization, thus producing a white fibrinous pellicle which is a constant accompaniment of this lesion.

Virchow[1] says of *hyperinoses:* "I do not think that we are enabled to conclude that, in a person who has an excess of fibrine in his blood, there is, on that account, also a greater tendency to fibrinous transudation, on the contrary, I should rather expect that, in a patient who produces at a certain point a large quantity of fibrine-forming substance, much of it would pass from that point into the lymph and finally into the blood. The exudation, therefore, in such cases, may be regarded as the *surplus* of fibrine formed *in loco*, for the removal of which the lymphatic circulation did not suffice. In accordance with this view, then, the mucous patch may be considered the accidental focus of development of the syphilitic corpuscle, resulting from superficial lymphatic capillary obstruction caused through a hyperplasia excited by the presence and usual influence of the syphilitic disease-germs. Mucous patches always occur upon mucous membranes richly endowed with lymphatic vessels, as upon the soft palate, the tonsils, the gums, the inner surfaces of the cheeks, the tongue, the walls of the pharynx, etc..

The mucous tubercle, which is the analogue of the mucous patch, occurs upon the integument at points also of unusually abundant lymphatic distribution, as about the vulva, the anus, on the scrotum, about the nails of the fingers and toes, etc.

A specific exudative inflammation of the eye is a not infrequent occurrence in early constitutional syphilis. The late Prof. Von Graefe made the statement that, according to statistics collected by him, about sixty per cent. of all cases of iritis occur in persons affected with syphilis. Dr. Bumstead[2] says: "We have no certain means of distinguishing syphilitic iritis from that dependent upon injury, rheumatism, or other causes," and thus describes the pathological changes which occur in the course of this affection in syphilis:

"At an early stage of the disease, the pupil assumes a dull appearance, owing to commencing changes in the anterior capsule of the lens; it may also be somewhat irregular. This irregularity of outline due to adhesion between its margins and the capsule of the lens, or to exudation into its substance, becomes more marked as the disease progresses, and is especially evident if the pupil be dilated by belladonna or atropine. When its margin is found to be scolloped, owing to its being attached at some points and drawn out in others, in some cases the adhesions become continuous around the whole circumference, and the capsule of the lens is covered with a layer of lymph which completely blocks up the pupil."

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[1] "Cellular Pathology," p. 199.

[2] Bumstead on "Venereal Diseases," third edition, p. 660, 1870.

This inflammation may be serous instead of plastic, or it may assume a suppurative form. It is associated with the period of roseolous or papular eruptions, mucous patches, etc. Carmichael states that the eruption usually associated with it is the papular.

Recent observations have shown the presence of an extensive distribution of lymphatic vessels in every structure and tissue in the human organism. Everywhere that blood-vessels are present, an accompanying system of lymph-vessels is also found, thus going far to confirm the recent views of Willis, Recklinghausen, and others, of the office of the lymphatic system as a necessary adjunct to the blood vascular system, for the purpose of gathering in and returning to the general circulation the excess of lymph exuded for purposes of nutrition. With this view of the office of the lymphatic system, it is easy to account for many diseased conditions heretofore considered anomalous, to appreciate the causes of the multiform lesions in syphilis which have hitherto puzzled acute clinical observers, and conveyed to them the impression that the disease was developed through no fixed laws, but resulted from a general and almost necessarily permanent vitiation of the entire affected organism.

The structures of the eye, especially the more delicate, as the iris, the retina, the conjunctiva, etc., have hitherto been considered as entirely deficient in a lymphatic circulation. Schalbe[1] has, nevertheless, fully demonstrated their presence in all these structures, and has shown an especially generous distribution to the iris, and in excess of the neighboring tissues. With the presence of the vitiated germinal element free in the general circulation, with its known power to cause obstruction to the lymph circulation, and a consequent hyperplasia, what should we expect to find? As a natural sequence, we should expect what we find in the advance and course of syphilitic disease, viz., that this delicate structure often suffers, and through the same causes that result in hyperplasias, in other parts richly endowed with lymphatic vessels.

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[1] "Handbuch der Lehre von den Geweben der Menschen und der Thiere," by Stricker, Leipsic, 1872.

A very general impression prevails that there is an entire absence of the lymphatic distribution in the osseous structures. Cruikshank, many years since, claimed to have discovered the entrance of a lymph-vessel into a dorsal vertebra. More recently, this observation has been confirmed by Soemmering and Bonamy. Sappey and Gross have demonstrated the presence of lymphatic vessels in the marrow of the tibia. Bonamy states that he injected the lymphatics which arise from bones by little foramina which are found on the side of the internal condyle of the femur. Accepting these statements, we find a reasonable explanation of the occurrence of ostitis and periostitis, and the formation of nodes, which are found associated with the early or active period of syphilis.

Not alone in the structures of the eye, and in the bones, in the muscles, and in the serous membranes generally conceded to be outside the track of the lymphatic distribution, are these essential vessels now ascertained to be present. Even in the fibrous and cartilaginous tissues—those most scantily supplied with blood-vessels of all the normal structures—these vessels have been absolutely demonstrated. The recent work of Ludwig and Schweigger-Seidel, published in Leipsic, 1872, is rich in illustrations of the presence and distribution of lymph-vessels in cartilages and fasciæ. Through the kindness of the President of this Society, Dr. Jacobi, I am now able to exhibit to you this very admirable and elaborate work, received during the last month from Leipsic.

Time does not, at present, allow me to extend the consideration of the subject of the evening further, but on a future occasion I confidently expect to be able to demonstrate the intimation conveyed to you at the close of my previous paper on syphilitic infection, viz., that many if not all of the later manifestations of syphilis may be satisfactorily explained through causes wholly dependent upon interference with the lymphatic circulation, and that it is to deposits of fibrine, organized through the syphilitic influence, in and around lymph-vessels and lymph-sacs, in the earlier stages of the disease, and its subsequent contraction, that the various external and internal lesions occurring in the later stages of syphilis are chiefly due.

And, still further, I believe that the management of this disease is capable of being established upon an intelligent and philosophical basis; not necessarily changing the present recognized methods of treatment, but by affording a reasonable explanation of the manner in which known, effective, remedial measures result in the alleviation and cure of syphilis, and thus aid to some degree in extricating the *prophylaxis* and the *therapeutics* of this disease from the slough of empiricism in which it has hitherto been submerged.

Clinical Records from Private and Hospital Practice.

I.—*Prediction of Sex in Utero by Auscultation.* By T. J. Hutton, M. D., Resident Physician, Long Island College Hospital, Brooklyn, N. Y.

The pulsations of the fœtal heart were first detected by M. Mayor, of Geneva, and in 1818 he published his discovery.

For some time afterward the subject seems to have been forgotten, and, in being revived by Kergaradec, was practised up to a recent date simply for the purpose of determining pregnancy in doubtful cases. Within the past few years, however, the assertion has been advanced by Tyler Smith and others that, not only can pregnancy, in its advanced stages, be determined by auscultation, but also the presentation, position, and sex of the fœtus *in utero*.

While, then, utero-fœtal auscultation, as a means of determining pregnancy, has been a part of obstetric literature for nearly fifty years, that part of it which proposes to determine the *presentation*, *position*, and *sex* of the child, previous to labor, is comparatively new, perhaps not generally known, and doubtless but little practised. In considering these modern claims let us inquire, May we always detect the fœtal pulsations? If so, of what practical value? "It is a very rare circumstance," says M. Depaul, "for the pulsations of the fœtal heart to be inaudible during the last three months of gestation, unless the child be dead." They failed to be detected in but eight cases out of nine hundred and six, examined at this period. Of what practical value? "A discovery of *very great* value to the accoucheur," says Tyler Smith (p. 25); "a means of diagnosis never to be neglected," says Cazeaux (p. 257). "I have repeatedly found auscultation of incalculable value, and have been, I believe, enabled by its evidence alone to save, in several instances, the life of the child," says Simpson, in referring to malpositions and tedious labors ("Obstetrics," pp. 422, 487).

Its Value in determining Sex.—All the difficulties and dangers of labor, all *post-partum* complications, instrumental deliveries, fœtal and maternal deaths, occur much more frequently in *male* births; the ratio of their frequency, as compared with the birth of females, is nearly as three to one (ibid., pp. 309, 315). In *foreknowing* sex—if such be possible—may we not hope to anticipate, abbreviate, or prevent these justly-feared accidents of the puerperal state?

Again, any just means that shall prove a master-key to confidence and respect, any new acquisition of skill which will exalt the physician and his profession in the estimation of the people, must be worth acquiring. "A prominent medical gentleman of this city, in attendance upon a lady in her confinement, in which, by examination, he found by anus, tuber ischii, scrotum, and penis, there was a *son* to be born, notified the father accordingly, and, of course, a son was born. A few days after, the unlearned but delighted father sent the physician a check for five hundred dollars, remarking that any physician deserved that sum who could tell the sex before the birth" (*vide* Swayne's "Obstetric Aphorisms," p. 69). In the present article, suggestive rather than exhaustive, I present to the profession the results of some observations in this direction, few in number, it is true, but made, as will be noticed, at widely different periods, and with one and the same result. The rule employed is a modification of those advanced in recent text-books, viz.: *Fœtal pulsations, heard below a horizontal line, dividing the uterus into two equal parts, denote vertex presentation; above it, breech presentation; below this line, and to the left, first position; below it, and to the right, second position. When the fœtal pulsations number* 144 *per minute, it is a female;* 124 *per minute, male.* And, as deviations from this rule, or average, will doubtless be encountered, I venture to add that a variation of *six beats per minute*, from 124 upward, or from 144 downward, will not endanger a diagnosis, provided auscultation be practised in the *ninth month of pregnancy.* And as a proof of the practicability and reliability of this rule, as a means of diagnosticating sex *in utero*, I submit the essential points from the clinical record of seven cases, delivered in this hospital, the only ones in which I had an opportunity of testing it, in which it was *the sole guide*, and without a *single failure:*

Case I.—Mary M., aged nineteen, United States, domestic, single, primipara; admitted to hospital August 10, 1871. *Parturition.*

General health good; no albumen in urine; saw last menses November 17th. Counting back three months and adding seven days, labor will be due August 24th. Pulsations of fœtal heart heard by ear and stethoscope, in left inferior abdominal region, 124 per minute; hence diagnosis, male; vertex, first position.

August 25th.—Was delivered of a male child at 5.30 P. M. Vertex presentation; first position.

Even before "she is delivered of the child, she remembereth no more the anguish, for joy that a *man* is" (to be) "born into the world."

Case II.—Alice F., aged twenty-one, Ireland, domestic, married, multipara; admitted August 11, 1871. *Parturition.*

General health good; no albumen. Date of last menses forgotten. Fœtal heart pulsates in left inferior abdominal region, 144 per minute; diagnosis, female; vertex, first position.

August 26*th.*—Fœtal heart, 137 per minute. Labor began at 4 P. M., ended at 9.15 P. M.; diagnosis, female; vertex, first position. During labor the fœtal heart was slightly accelerated.[1]

Case III.—Delia H., aged twenty-three, Ireland, domestic, primipara; admitted August 25, 1871. *Parturition.* Date of last menses forgotten.

General health good; no albumen. Fœtal pulsations in left inferior abdominal region, 127 per minute; diagnosis, male; vertex, first position.

August 26*th.*—Labor terminated at 6 A. M.; male; vertex, first position.

January 5, 1872.—Baby being sick, this patient travelled seventeen miles, five of which she walked, to see "her own doctor;" predicting sex had inspired such confidence.

Case IV.—Amelia A., aged twenty-three, United States, house-keeper, multipara; admitted August 29, 1871. *Parturition.* Date of last menses forgotten.

General health pretty good; slight traces of albumen. Fœtal pulsations in *right* inferior abdominal region, 128 per minute; diagnosis, male; vertex, second position.

September 13*th.*—Fœtal pulsations 125 per minute. Labor terminated at 7 P. M., male; vertex, second position.

Case V.—Annie C., aged twenty, bank-note stamper, single, primipara; admitted April 25, 1872. *Parturition.*

Saw last menses August 8th; labor due May 15th. Health has always been poor; slight traces of albumen. Fœtal pulsation in the left inferior abdominal region, 150 per minute; diagnosis, female; vertex, first position.

May 16*th.*—Fœtal pulsations 144 per minute; labor terminated at 10.45 P. M.; female; vertex, first position.

Case VI.—Annie M., aged twenty-eight, Ireland, domestic, primipara; admitted April 30, 1872. *Parturition.*

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[1] In this case the reduction of the fœtal pulsations six beats per minute, in the two weeks preceding delivery, confirms my previous statement, that they *may* thus vary, and yet a diagnosis be made with almost absolute certainty. It also disproves the statement of some authors that "the frequency of the pulsations does not vary with the age of the fœtus. (*Vide* Loomis's "Physiological Diagnosis," p. 148; Cazeaux, p. 254.)

Patient enjoys good health; no albumen. Fœtal pulsations in left inferior abdominal region, 126 per minute; diagnosis, male; vertex, first position.

May 27th.—Labor terminated at 8.10 A. M.; male; vertex, first position.

CASE VII.—Kate C., aged twenty-four, Ireland, domestic, primipara; admitted May 7, 1872. *Parturition.*

Is in good health; no albumen. Date of menses forgotten. Fœtal pulsations in left inferior abdominal region, 124 per minute; diagnosis, male; vertex, first position.

May 30th.—Labor terminated at 8.30 A. M.; male; vertex, first position.[1]

In conclusion, I would add that, while these observations open up an interesting field of inquiry, and, so far as my observations have gone, have been *uniform in result*, still, from their small number, I only claim value for them as they confirm the observations of others. The cases were most carefully analyzed, and I submit them to the profession, more for the purpose of attracting attention to the subject than for any supposed value that may be attached to the limited number of the observations themselves. *Uniformity* in the cases observed is of value, especially when they confirm the observations of others.

II.—*The Mono-bromate of Camphor in Delirium Tremens.*

By ALLAN McLANE HAMILTON, M. D.

MY attention was called to this extremely valuable drug by my friend Dr. Hammond, in a recent article that appeared in this JOURNAL, and I have the satisfaction of attesting its merits in a case of delirium tremens I have lately treated.

Mr. E., a very plethoric, large man, of intemperate habits, after a prolonged debauch of several weeks, was lately prostrated by a very severe attack of *mania a potu*. I was called to him early one morning, and found him in an acute state of alcoholism, with great cerebral congestion, tremulousness, and great jactitation of the limbs. His conversation was muttering and incoherent, and his pulse full and soft. He had not slept for several nights, and of course was utterly unnerved. My first prescription was bromide of sodium in doses of twenty grains, repeated every two hours till he had taken nearly one hundred grains. This calmed him somewhat, but did not produce sleep. I found him the same night in a state somewhat more composed, but still restless—the pulse had moderated, and was irritable and small; and the convulsive movements had partially disappeared. His bowels were confined, and I ordered for him an enema, a bottle of citrate of magnesia, also a drachm of tincture of lupulin and tincture of hyoscyamus, with forty grains of the bromide of sodium. This latter prescription did not overcome the wakefulness; so at length I had a number of pills prepared after the following prescription:

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[1] Retracted nipples in this patient were developed by a chemical test-tube, from which the air was exhausted by alcohol.

℞. Camphoræ mono-bromat: ʒi; confectio rosæ, q. s. M. ft. massa and divide in pil. No. xij. One pill was given at 12 o'clock, and within a half hour the patient fell asleep, and slept soundly till the next afternoon, when he awoke refreshed, and drank some beef-tea and milk. At 12 o'clock the same night he took another pill, and slept till 3 o'clock A. M., when another was adminstered. From this medicine he obtained regular sleep and awoke always bright and refreshed. No bad effects followed, even when he took ten grains. His appetite is restored, and his nervous system seems to have experienced a complete rest and invigoration. I have tried this remedy in chordee, and am convinced it excels any combination of camphor and opium, or any of the usual medicines, administered in this complication. The high price will prevent it from being used as freely as some other medicines, as the chemists who manufacture it declare that the combination of the equivalents, camphor and bromide, in nearly every instance is attended by spontaneous ignition. Messrs. Caswell & Hazard, and Neergard, I believe, are the only pharmaceutists who dispense it.

III.—*Two Cases of Tape-worm consequent upon the Use of Raw Beef.* [Rundsch. Med. Chir., xiii., i.]

Upon the suggestion of an article in the *Rundschau*, December, 1871, by Dr. Levi, on the occurrence of tænia after the use of raw beef, Dr. Viezzoli, of Trieste, communicates two cases from his own practice which are here briefly reproduced; since it appears quite worth while to investigate the possibility of development of the above parasite after the continued use of raw beef. It may be remarked at the same time that Cruveilhier assumed an increase of tape-worms in dogs after a vegetable diet:

1. The first case occurred in a young woman, aged nineteen, who from simple inclination consumed about a quarter of a pound of raw beef daily for several months. Soon there appeared in the fæces traces of tænia mediocanellata, which disappeared upon the exhibition of koussou.

2. A boy four years old, upon the recommendation of his physician, on account of a long-continued diarrhœa, was fed with minced raw beef for about two months. As the boy continued to fail notwithstanding, he was brought to the hospital at Trieste, under Dr. Viezzoli's care, and as soon as symptoms of tænia appeared they were in this case also relieved by koussou.

IV.—*Primary Croup of the Nasal Mucous Membrane.* [Jahrb. für Kinderk., 314, 1871.]

Schuller reports the following case, which had an unusual course: A boy five years old, nursed by a wet-nurse, of healthy parents, who had suffered from catarrh from his birth, was affected with dyspnœa and convulsive attacks. A floating croupous membrane was discovered in the left nasal cavity; no fever, no cause for diphtheria or coryza syphilitica to be found. The croupous membrane was removed, but returned in two days, and after this also had been removed a copious hæmorrhage followed from the nose. Simultaneously there appeared, at the right under eyelid, a red spot the size of a hazel-nut, which disappeared the next day; when another appeared, with symptoms of fever, in the vicinity of the cheekbone, which, during the following days, extended in all directions over the surface of the skin, with symptoms of dermatitis (erysipelas). The croupous process also extended through the nose to the throat. The child died in a convulsive attack.

V.—*Case of Vicarious Menstruation.* By Dr. P. Megnet. [Lyon Médicale, Mars, 1872.]

Henrietta M., aged seventeen, well formed, although of a lymphatic constitution, is admitted into the Hôtel Dieu, February 24, 1862. This young girl says that, for eight months, she has had every month a rather abundant loss of blood from the breasts, sometimes on one side, sometimes the other. This hæmorrhage is always preceded by pain and turgescence of the breasts for a day or two. As soon as this loss of blood is terminated the cracks of the nipples close, and the breasts become soft again. This young girl has never menstruated; however, at the time of the hæmorrhage, she experiences a little pain, with a sensation of fulness in the abdomen and lassitude in the thighs.

The general health is very good. We have seen her three times at her periods, and have witnessed the phenomena above indicated.

The quantity of blood secreted was from thirty to forty grammes a day, lasting two or three days. Two months after her discharge from the hospital her menses appeared in the natural way, and since that time she had no more bleeding from the breasts.

Henrietta M. is now twenty-seven, is rather robust, but menstruates very irregularly every two or three months, and with considerable pain in the back and abdomen. She has an intercostal neuralgia, and some palpitation. In a word, she presents numerous symptoms of chlorosis, but without *bruit de souffle vasculaire.* She has been married for three years, and has had no children.

Bibliographical and Literary Notes.

ART. I.—*Untersuchungen über die embolischen Processe*, von Dr. JULIUS COHNHEIM. Berlin, 1872. Verlag von August Hirschwald.

THE writer states, in the preface to his book (entitled "Examinations into the Processes resulting from Embolism"), that it has not been his intention to present an exhaustive treatise upon the subject of embolism, but only to examine into the various questions belonging to this process, in the light of actual experiment.

The work is made up of four chapters:

I. The Disturbances in the Circulation occasioned by an Embolus.

II. The Importance to the Circulation of the Integrity of the Vessel-walls.

III. The Pathological Sequences of Infarction.

IV. Abscesses resulting from Emboli.

The organ he has employed, in studying the changes in the circulation produced by the embolus, is the frog's tongue, because of the convenient arrangement of its blood-vessels, and because it may be so readily placed under the microscope. The fluid used to create the embolism is an emulsion of wax, colored black by admixture with soot. The frog, having been partially brought under the influence of woorara (in order to allow of the greatest possible expansion of the tongue), one of two methods may be resorted to, in order to introduce the wax into the circulation. Either the heart may be laid bare, and, through a wound in its apex, the fluid injected into the aorta by means of a Phavaz syringe, and then the wound closed, by passing a ligature around the apex; or, the left aorta itself may be opened, the emulsion thrown directly into it, and then the vessel ligated. If the fluid be carried into the lingual vessels, the left side of the tongue is seen (microscopically) to become pale, and muscular twitchings are observable, the right half of the tongue remaining quiet. By small magnifying power (Hartnack oc. 3, obj. 2 or 4) it is easy to discover the point of embolism, by following the current of blood, either toward or away from the heart. Suddenly you find an artery through which the flow of blood is much more rapid than elsewhere; tracing this vessel toward the periphery, you soon arrive at the point of obstruction. The increased pressure in the collateral circulation, occasioned by this obstruction, causes increased rapidity in the current of blood, observable also in the corresponding veins. In the frog spontaneous contractions and dilatations take place in the lingual arteries, so that an embolus will sometimes, after remaining fixed for a length of time, move forward for a considerable distance. The blood never coagulates (in the frog) in the neighborhood of the embolus.

If the artery containing the embolus be brought under the microscope, it will be sure to be bulged out at the point where the wax is lodged, while in front of and behind this point its calibre seems to be diminished. The circulation has ceased entirely, in front and behind, as far as the first branch-artery. Behind the embolus the vessel is distended by motionless red and white corpuscles, or, more frequently, with plasma containing white corpuscles only; in front of the embolus, a similar condition exists. If the obstructed vessel be an "end-artery" (Cohnheim gives this designation to a vessel having no anastomotic branch beyond the site of the embolus), the blood remains motionless beyond the embolus; but, should communicating branches exist beyond this point, the collateral circulation soon becomes sufficient to repair the defect produced by the interruption to the flow of blood, except in that portion of the artery between the plug and the first arterial branch in the direction toward the heart, which, of course, must continue isolated. If we study the phenomena taking place in an end-artery, we observe that the blood is motionless throughout the remainder of the vessel, in the capillaries, and in the corresponding veins, until they meet other venous connections. Presently, however, this stasis gives place to a *backward flow*, beginning in the veins at the point of union between the stagnant and moving blood, and extending gradually through the capillaries back into the artery—the more readily, if the artery have been comparatively empty of blood. With this backward flow a *to-and-fro* movement soon becomes visible. After several hours the tongue upon the affected side assumes a dark-red color, apparent to the naked eye. The explanation of this *backward flow* is very simple: Beyond the point of obstruction in the artery, the pressure is *nil*, in the anastomosing veins; though feeble, it is always positive, until the obstructed vessels be again filled with blood, when the to-and-fro movement begins. Upon the third or fourth day (the frog being constantly under the influence of woorara), hæmorrhage occurs, first from the capillaries in the papillæ, later from the larger capillaries. This extravasation is not complete before the end of several days, when, at the affected point upon the tongue, a dark-red, wedge-shaped spot may be recognized by the unaided eye, the hæmorrhagic infarction. Sometimes hæmorrhages follow upon embolism even of the capillaries, but this is not the rule.

The vessel-walls may practically be said to be normal when they perfectly perform their function. In order to ascertain the effect upon the blood-vessels of shutting off the flow of blood through them, Cohnheim again had recourse to the frog's tongue, the result being the same whether the animal were under the influence of woorara or not. A ligature was passed around the base of the tongue, a narrow strip of leather being inserted underneath it to prevent cutting through the tissues. The effect of thus interrupting the circulation was in exact proportion to the length of time the ligature was allowed to remain. Of course, all flow of blood ceased. The difference in color of the arterial and venous blood remained, the tongue lost its glistening appearance, and became of a dull, pale-blue color. After three or four days the ciliated epithelium ceased all movement, separation and disintegration of the muscular fibres took place; the nerve-fibres and the cells in the connective tissue underwent no change. If the ligature were removed within twenty-four hours, the blood would rush with great rapidity into the arteries, through the capillaries, and into the veins, without encountering anywhere any obstruction, the vessels being largely dilated. In the course of two or three hours the vessels would contract (the arteries first) to their normal size, and the tongue would reassume its natural appearance. The result was very different, however, if the ligature had been allowed to remain for forty-eight hours. Even then, as a rule, the blood would flow without interruption into and through the vessels; or it could be induced to do so by gently rubbing the tongue at the site of the ligature. But soon the arteries would begin to contract (thus causing a slowing down in the current of blood), the veins remaining dilated; the white corpuscles would arrange themselves alongside of the walls of the veins, and begin to be extravasated, the capillaries also would take part in this extravasation, red corpuscles being mingled with the white. The fluid portions of the blood forsook the vessels, and the tongue became very œdematous. By the end of the second day this transudation has nearly ceased; meanwhile the white corpuscles had begun to be absorbed by the lymphatics, and the œdema had commenced to disappear; the veins gradually returned to their normal size, and the entire organ resumed its healthy appearance, except the muscular fibres, which never entirely regained their tone; and occasionally a collection of white corpuscles, simulating a kind of abscess, would be recognizable for a length of time.

Should the ligature remain for sixty hours or longer, upon its removal, very copious hæmorrhage from the capillaries would follow, the red corpuscles in large quantities transuding the walls of the veins as well as of the capillaries. If the ligature were removed upon the fifth or sixth day, the circulation was never resumed—the epithelium would be separated in patches from the tongue, the muscles were completely disintegrated, the aspect of the organ was dull, whitish, not transparent, sometimes greenish; in a word, all the symptoms of wet gangrene were present. Similar experiments were made by Cohnheim upon the lungs and mesentery of frogs, with similar results. But these experiments upon mammals were much more striking. He has employed the external ear of rabbits, inserting a cork plug into the meatus, and then applying a ligature to the ear. He did not shave the ear. The ligature being removed after eight or ten hours, the blood would rush quickly through the vessels; after a while œdema would set in; multitudes of white corpuscles would be exuded into the connective tissue. When the removal of the ligature took place at the end of twenty-four hours, hæmorrhage always followed; the ear became swollen, red patches appeared, gradually uniting and forming hæmorrhagic infarctions; suppuration would begin, and the ear be lost. The ear of the dog or Guinea-pig, or the scrotum of the rabbit, may be used with advantage in these experiments. Cohnheim has also ligated the renal artery and vein in the rabbit, allowing the ligature to remain for two hours. Upon its removal the organ quickly became swollen to twice its normal size, the tissue being infiltrated with blood, the urine bloody. Under the microscope the capillaries of both the medullary and cortical portions were enormously distended with blood-corpuscles. Necrosis of the organ followed at certain points, while at others infarctions resulted. Never, after the above experiments upon the frog's tongue, or the rabbit's ear, was Cohnheim able to detect any change in the structure of the blood-vessels. Whether injury was caused to the vaso-motor nerves it was impossible to ascertain with certainty. But, since this transudation of the blood-corpuscles through the vessel-walls takes place after the flow of blood through the vessels has been stopped for a length of time (reasoning from the analogy of the muscles and nerves, which lose their irritability when their circulation is interfered with) we may infer that the integrity of the walls is disturbed and injured by shutting off the circulation through the vessels. The first indication of this disturbance is the general dilatation of the vessels, caused by a paralysis of the circular muscular fibres, which fibres may become completely disorganized.

The infarctions, occurring after embolism of an end-artery, are occasioned by the changes which the vessel-walls (and especially the capillaries) undergo in consequence of the interruption to the circulation. The infarctions are the result of the combined action of the necrosis caused by the cessation of the flow of blood to the organ, and of the heaping up of the corpuscles, caused by the retrograde circulation from the veins corresponding to the artery in which the embolism is located.

In reasoning from these experiments upon frogs to the human subject, we must bear in mind the differences in organization as seen in the calibre of the vessels, the pressure of the column of blood, and, above all, the intimate relation to each other of the separate apparatuses in all warm-blooded animals. Only extremely seldom has Cohnheim discovered ecchymoses in the organs of warm-blooded animals behind *capillary* emboli.

One of these results always follows embolism of an artery in the human subject. Either the embolus remains stationary until it becomes organized and united to the wall of the artery, with no perceptible injurious influence upon the organ supplied by the artery resulting; or, beyond the embolus necrosis occurs, manifesting itself as deep or wet gangrene, or as softening; or hæmorrhagic patches develop, which, in superficial organs, assume the appearance of infiltrations in parenchymatous organs, as infarctions.

Examples of the first result are most frequently met with in the lungs; the skin and brain furnish the most common instances of the second, while the hæmorrhagic patches are most usually seen in the lungs, spleen, and kidneys; less frequently in the brain, eye, and intestines. The organs in man which possess the end-arteries are the brain, retina, lungs, kidneys, and spleen. The veins in these organs are destitute of colors, thus admitting of the retrograde circulation; and in these organs we find hæmorrhagic infarctions in conjunction with emboli of their arteries. In the larger end-arteries the coagulation of the blood plays an important part, the clot (when it exists) preventing the retrograde flow of blood; and hence the formation of an infarction, until, finally, it undergoes, with the vessel-wall, disintegration. The situation and position of the organ in which the embolus occurs influence the formation of the infarction, since the retrograde flow of blood takes place much more readily when aided by the force of gravity. If the embolus only partially fills up the calibre of the artery (instead of completely plugging it) a necrosis may result, if the supply of blood be insufficient for the nutrition of the organ, but never can an infarction follow.

It will be seen from the foregoing that two conditions are indispensable for the formation of an infarction, viz.: the embolus must lodge in an end-artery, and the corresponding veins must be without valves. But the above-mentioned additional conditions must exist as well. The lungs supply these conditions more fully than any other organ, next the spleen, afterward the kidneys, brain, and retinæ. The peculiar distribution of the blood-vessels in the liver explains the fact that hæmorrhagic infarctions are never met with in that organ.

Abscesses following embolism are always occasioned by the septicæmic character of the embolus itself. These abscesses, when of any considerable size, are located either in the lungs or liver. Particles of foreign matter in the arterial circulation (except in the rare cases of thrombi from the pulmonary veins) are due to inflammation of the lining membrane of the heart or arteries; they are always very minute, and hence can obstruct only the smallest arteries (as of the brain, kidneys, etc.), thereby producing only very small abscesses. The venous circulation through the liver and lungs, on the contrary, is very often charged with large particles of septicæmic matter, which, being detained in the larger vessels, naturally cause abscesses of larger dimensions. In the liver we find abscesses only, never infarctions; in the lungs we find both; the infarctions being situated toward the periphery, their apex toward the hilus of the lung, while the abscesses are always formed in the neighborhood of the hilus, and are usually spherical in shape. Infarctions result from obstruction in the calibre of an end-artery; abscesses are formed when a vessel (not an end-artery) becomes plugged by some septicæmic substance. The process developed by means of such a substance is purely inflammatory, the various stages of inflammation being often visible in different portions of the organs in which the abscesses are found.

Cohnheim has observed the development of such abscesses in the rabbit's ear, a week being sometimes required in this animal before the abscess is fully formed. He describes in his book the process at length. He has satisfied himself by experiments that, in this process, the nerves exert no important influence.

Art. II.—*Observations pour servir à l'Histoire de l'Ictère Grave. Par* M. Picot, *Professeur d'Histologie à Tours. Journal de l'Anatomie et de la Physiologie* (Robin's), No. 3, 1872.[1]

Leontine A., aged twenty-two years, entered hospital at Tours April 29, 1871 (vaginitis, ulcerations of cervix, ulcerating bubo of right inguinal fold; mucous patches upon internal surface of cheeks, and upon the borders of the tongue).

April 31*st.*—Appearance of mild icterus; no digestive troubles; augmenting pains in the hepatic region; patient obliged to keep in bed. No increase in volume of liver, simply great pain upon pressure. Continuance of condition up to August 1st, when the secondary manifestations yielded to treatment. An increase in size of the hepatic gland was now detected (two fingers' breadth in all directions). On the same day the patient appeared very much enfeebled, and had an attack of syncope.

August 3*d.*—Liver excessively painful upon pressure, still further increased in size, intense icterus, decolorized fæces, urine containing a great abundance of the coloring-matter of bile. General sensibility exaggerated, hyperæsthesia such that the slightest touch excited the complaints of the patient; pupils dilated; artificial light appeared yellow to the sufferer. Patient does not see the surrounding attendants. Sense of hearing intact. Paralysis of intelligence. Replies to questions embarrassed. Patient repeats a great number of times the same word, the same phrase, always in the same tone, the same accent; slight delirium; motor power diminished. Pulse 84, small. Temperature 99.5°. During the night of 3d and 4th, extreme agitation. Patient cries out, attempts to throw herself from bed; and says she sees fire, and that she is going to be burned. Profound coma followed, with total loss of sensibility; reflex movements in limbs; pupils dilated, and unaffected by light. Respiration normal; pulse 118, small; temperature 99.6°. Enormous diminution in volume of liver, so that it possessed its normal size.

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[1] Experimental Researches into a New Excretory Function of the Liver. American Journal of the Medical Sciences. Philadelphia, October, 1862, New Series, vol. xlvi., p. 305, *et seq.* Translation into French, published by J. B. Baillière, in 1868; "Honorable Mention," with a "Recompense" of 1,500 francs from the Institute of France, Académie des Sciences, concours Montyon, Médecine et Chirurgie, in 1869.

August 4th, at 4 P. M.—Appearance of dark-colored, fetid vomiting, followed by a discharge of the dark matter from the nose and mouth. Death, the following day, at 12.30 M. Previous to the end, pulse 148; respiration 42, and two hours before death temperature reached 101°.

Autopsy: ecchymoses over the epiploon, mesentery, peritonæum, and intestine; arborization of parietal fold of peritoneum, intestine, and stomach. *Liver*, reddish-yellow color, firm, weight 25 ℥. Injections of water demonstrate permeability of the hepatic ducts, the portal vein, and hepatic artery. *Pancreas*, healthy. *Spleen*, enlarged, softened. *Kidneys*, normal in size, tissues softened, stained a greenish-yellow. Some hæmorrhagic spots in cortical substance of right kidney. *Lungs*, congested. *Heart*, flaccid, pale, friable; dark reddish-brown tinge about the mitral valve. *Brain*, no lesions. *Blood*, thick, viscid, dark-colored.

Microscopic Analyses.—Blood, astonishing diminution of red corpuscles, which are small, and have a stellate form. White corpuscles, not increased in number. *Liver*, disappearance of liver-cells, which are replaced by fatty granules, and shiny globules having the appearance of fat; latter appear to be liver-cells, diminished in size, which have undergone fatty degeneration. Enormous development of interlobular tissue. Bile-ducts enlarged in parts, and in others much constricted, containing dark granules suspected to be cholesterine.

Chemical Analysis.—This was undertaken to verify the results of Prof. A. Flint, Jr., relative to the existence of cholesteræmia. *Examination of Blood.* Found to contain of cholesterine 1.804 per 1,000. *Examination of Brain.* Found to contain of cholesterine 15.2 per 1,000.

Reflections.—Syphilis is known to determine in most organs an augmentation of connective tissue. At first the increase in volume of the connective tissue exceeds that which it possesses when its development is complete. We have therefore in these morbid processes two distinct periods: the one characterized by an increase of the organ affected, the other by atrophy. In the special case under consideration there was this remarkable, viz.: the rapidity with which the retraction of the connective tissue of new formation took place. Involuntarily, in presence of the simultaneous grave symptoms which manifested themselves in the nervous system, one would be led to think of acute yellow atrophy of the liver. But the firmness of the liver-tissue forbids this assumption. If I should wish to characterize the lesion, I think I should give it the title interstitial hepatitis at the stage of acute atrophy (hepatite interstitielle à phase atrophique suraiguë).

The icterus requires consideration. The bile-passages were pervious, and it was only by microscopic examination of the lesion that the retention of bile, and the passage of its coloring-matter into the blood, became comprehensible, viz., to the narrowing and obliteration of the bile-ducts by the enormous development of connective tissue of new formation around them.

The diminution and deformation of the blood-corpuscles are directly attributable to the action of the biliary acids, as the destructive action of these acids has been demonstrated by Plattner, Kühne, and Hoppe-Seyler.

From a chemical point of view the blood presented a very important lesion in the increased quantity of cholesterine. In the venous blood of the arm we find in a healthy person .625 per 1,000, but in this case I found 1.804 per 1,000. To be sure, M. Flint has shown that the blood from the brain contains more cholesterine than that from the arm. My analysis was made from mixed blood, which would contain more than the above amount (.625 per 1,000), still the figures (1.804 per 1,000) are too large to be accounted for, even if the blood had been taken directly from the jugular.

The chemical analysis of the brain gives the explanation of the nervous manifestations. The largest quantity of cholesterine found in health by M. Flint was 11.456, while here the quantity was 15.200.

Physiological Remarks.—These points settled, I shall try to explain physiologically the nervous symptoms manifested during the disease. The dissolution of the blood-globules occasions a deficiency of oxygen, and consequently the encephalon is deprived of one of the essential materials for its nutrition. On the other hand, cholesterine, which the labors of M. Flint have shown to be the product of disassimilation of the brain and nervous tissue, no longer eliminated by the liver, accumulated in the blood, and the brain received therefore for the requirements of its nutrition the products of its own distinctive metamorphoses. We know that the functions of anatomical elements in the organism are dependent upon the integrity of their nutrition to such an extent that, when the latter is interfered with, the functions are immediately disturbed. It is then this alteration in the nutrition of the anatomical elements of the nerves, to which the remarkable symptoms produced in the innervation are to be referred. But when one wishes to go further, and ascertain what *rôle* is attributable to the cholesterine, and what to the disappearance of the red globules; whether the biliary salts act by themselves as poisons to the nervous system, or only by their destructive action of the blood-globules—these questions it is impossible to decide in the present state of science. From what I have just said it appears that the opinion of M. A. Flint on cholesteræmia is well founded; but that we cannot attribute to it alone the pathological manifestations which take place in the nervous system, since the biliary acids and their derivations have likewise a deleterious influence upon the blood, and consequently upon the encephalon and other organs of the body.

ART. III.—*Polymorphous Balneology.* By LERSCH, Bath-Inspector at Aix-la-Chapelle. Erlangen, 1871, F. Enke.

THIS work forms the second part of the author's "Practical Balneology." The title Polymorphous Balneology is designed to express the various forms and materials of the baths, in contradistinction to the ordinary methods with simple or mineral waters. The materials of the baths treated of in the section of the work belong mostly to the organic kingdom, only a small portion being derived from the inorganic. Those baths prepared from vegetable or animal matters are prepared either entirely without water, as the baths of fatty oils, of fresh or dried leaves, of the refuse of the wine-press, of tan, etc., or are aqueous mixtures and solutions. Among these additions to baths, especial notice is taken of those containing protein, such as blood, milk, whey, the gastric and intestinal contents of animals, decoctions of meat, and also the farinaceous, such as starch, clay, malt, etc., and the glutinous, as the decoctions of mallows, and the like. Of the acid non-fluid additions to the baths especial notice is taken of the tannic and similar acids, as occurring in oak-bark, tan, the walnut, quinine, etc., with which are connected the ulmic acid of the turf and bog-earth, and finally the uric acid of guano. Latterly much use is made of substances which evaporate at a high temperature. To this class belong especially the ethereal oils, as occurring in the coniferæ and other plants, e. g., mustard-oil, oil of turpentine, and the allied rock-oil, and also some volatile acids, especially formic, as well as the acridity—chemically unknown—of sea-water. Thus the detailed account of the vegetable and animal baths comprehends a considerable theme, the investigation of which, the author remarks, is essentially facilitated by the fact that our positive knowledge of many of the varieties of these baths is still very meagre, and hence many sections of the subject have to be briefly touched upon. The inorganic kingdom is drawn upon in the materials for the sand-baths, as well-heated air, carbonic acid, and other gaseous and atmospheric baths, and in the aqueous solutions of many simple and compound bodies. In many of the earth, mud, and bog-earth baths, organic and inorganic matters are both represented. In each of the varieties of baths which are are so exhaustively investigated in this volume, the locality is indicated where they may be obtained. The work will be read with much interest, treating so fully as it does on a subject upon which too little is generally known.

Books and Pamphlets Received.—On Food, its Varieties, Chemical Composition, Nutritive Value, Comparative Digestibility, Physiological Functions and Uses, Preparation, Culinary Treatment, Preservation, Adulteration, etc. By H. Letheby, M. D., M. A., Ph. D., Professor of

Chemistry in the College of the London Hospital, etc. Second edition, enlarged and improved. New York: William Wood & Co., 1872.

Lectures on the Principles and Practice of Physic. Delivered at King's College, London. By Sir Thomas Watson, Bart., M. D., F. R. S., Physician-in-Ordinary to the Queen, etc., etc. In two volumes. From the fifth revised and enlarged English edition. Edited, with Additions and Numerous Illustrations, by Henry Hartshorne, A. M., M. D. Philadelphia: Henry C. Lea, 1872.

Historical and Biographical Memoirs, Essays, Addresses, etc., written at Various Times during the Last Fifty Years, and now first published in their Collected Form. By George B. Wood, M. D., LL. D., Emeritus Professor of the Practice of Medicine in the University of Pennsylvania. 8vo, pp. 576. Philadelphia: J. B. Lippincott & Co., 1872.

Sciatica, Lumbago, and Brachialgia: their Nature and Treatment, and their Immediate Relief and Rapid Cure by Hypodermic Injection of Morphia. By Henry Lawson, M. D., Lecturer on Physiology in St. Mary's Hospital Medical School, etc. London: Robert Hardwick, 1872.

Reports on the Progress of Medicine.

THEORY AND PRACTICE.

1.—*The Treatment of Hepatic Dropsy.* By W. R. Basham, M. D., Physician to the Westminster Hospital. [Practitioner.]

There is no form of dropsy more amenable to treatment, at its first occurrence or in its early stage, than hepatic dropsy, arising from engorgement of the liver, or even hypertrophy of that organ caused by the abuse of alcoholic drinks. It is a disease of frequent occurrence in our wards, and consequently repeated opportunities are afforded of measuring the influence of remedies, and laying down some general principles of treatment. The efficacy of treatment is proportioned to the stage or period in which it is commenced, as well as to the absence of any complication in heart or lungs.

If the accumulation of fluid in the belly be of some months' duration; if the anasarca of the lower extremities be on the increase; if the abdominal surface be tense and shining; if the marking of the wall by the inosculation of the superficial branches of the external epigastric veins with the external mammary veins be well marked; if the urine be scanty, loaded with lithates stained deeply with purpurine; and lastly, if the urine be, however slightly, albuminous, remedies are of no avail; the disease (cirrhosis) has reached its last stage, and no arrest in its progress can be expected. If, on the other hand, the dropsical state be recent; if the distention of the abdominal walls be moderate; if the anasarca of the lower extremities be trifling; if there be no evidence of enlargement of the superficial abdominal veins, and the urine be free from albumen—then the prospect of relief by appropriate remedies is encouraging, and often leads to an apparent cure of the engorged or hypertrophic state of the liver. The term *apparent* is designedly employed, because the record of many cases, in which the disappearance of the ascites, the restoration of the digestive functions, and the nutritive processes generally, sanctioned the result as one of present cure; yet probably from the return of the patient to the vicious habits which originated the disease, in a year or more the ascites returned, followed by evidence of a contracting or cirrhosed liver, and consequently a state of disorganization of structure hopelessly beyond all prospect of mitigation or relief.

The ordinary history of these cases of hepatic dropsy consists of an antecedent period of gastric disturbance, characterized chiefly by defective appetite, occasional retching, and some degree of epigastric tenderness on pressure. The most common symptom in reference to the stomach is morning retching or even vomiting; on first rising, a hacking, irritable kind of cough, without any expectoration, is followed by an effort of retching, with probably a copious expulsion from the stomach of a white frothy mucus: this represents a form of what may be appropriately termed gastric catarrh. In ninety-nine cases out of a hundred, the remedy to which the patient resorts, while it momentarily relieves the irritable stomach, perpetuates the original mischief, augments the vascular congestion of the gastric glands and mucous membrane, and leads still further to disorder in the hepatic circulation. The remedy thus all but universally selected by the patient or his friends is a stimulant in some form or other—rum-and-milk, gin-and-milk, gin-and-bitters, and a host of similar concoctions, familiar to the host and hostess of every public-house, and known to them to be in popular demand in the early hours of the day. This state of gastric catarrh, temporarily relieved by these drams, is but too often accompanied by an almost total inappetency and even distaste for solid food. Patients will often declare they have not eaten two ounces of solid food at any single meal for months. Drink is their only food. Flatulent distention of the bowels, sluggish action, and scanty, high-colored urine, continuing for some time, will mask the commencement of the presence of fluid in the belly; sometimes the patient complains of a heavy, dragging weight in the right hypochondrium, particularly if he reclines on the left side. Most of these symptoms pass by unregarded; they are not urgent enough to cause a cessation from work—and probably it is not till an increasing size in the abdominal girth, marked by the necessity of letting out the waistband, or stays, according to sex, that the patient seeks medical advice, and the real state of things becomes at once revealed. It is at this juncture, this early stage, that the greatest benefit is derived from active treatment. The use of all stimulants must at once be discontinued. The stomach should have absolute rest for a day or more: a blister to the epigastrium, kept open for a few days, tends materially to lessen the irritability of this viscus. The medicinal agents of more service at this stage are brisk mercurial purgatives. That valuable, though now too much neglected, preparation of calomel should be selected. Five or even ten grains, with half a scruple of the bicarbonate of soda and a little powdered ginger, should be given overnight, about twice a week, and a warm aloetic purgative in the following morning. If the bowels require still stronger purgatives, the calomel may be given in combination with jalap, or colocynth, to be followed by some saline mixture in the morning. Half a grain of podophyllin with colocynth is also most efficacious as a purgative. Two or three days' treatment on this plan will bring the digestive function to some degree of activity, and the patient will often say he feels an appetite and desire for food such as he has not known for a very long time. Great caution is needed in these cases to regulate the diet. In hospital practice this is not difficult, but in private life many circumstances combine to render strict attention to dietetic rule a troublesome matter. The all but innate belief among this class of patients in the harmlessness of the stimulants they take, and of their efficiency as remedies for almost every ailment, renders abstinence from them almost impossible, unless the patient be under the wholesome restraint of hospital treatment.

The treatment of the early stage of hepatic dropsy resolves itself into efforts to relieve the engorgement of the liver, to regulate the digestive function, and set free if possible the obstructed portal circulation. It has been already remarked that, if a case comes under notice sufficiently early, these objects are usually attained. It is not often, at least in hospital cases, that the patient comes under observation at so favorable a period. Mostly the condition of the patient represents a far more advanced stage of the liver-disease.

The ascites has been probably of some weeks' duration; the breathing has become affected by the abdominal fluid pressing upward on the diaphragm and thus limiting and embarrassing its movements. Bronchial catarrh, particularly in the winter months, increases the pulmonary distress. The heart's action is often irregular. The lower extremities are probably greatly œdematous; the urine is scanty, high-colored, with abundance of pink urates; and the vessel in which the urine stands is stained of a bright carmine red. The bowels are generally sluggish; there is no desire for food, and stimulants are alone cared for. This is a grave aspect of symptoms, and the prospect of relief is proportionately less.

There are cases, which experience will without difficulty discriminate, in which great advantage is derived from a small bleeding from the arm. Six or eight ounces at most may be taken, with speedy relief to the pulmonary distress, and a consequent subsidence of the venous tension, and a diminution of the excessive anasarcous effusion.

The indications by which the venesection may be justified are found in the aspect of the patient and the state of the pulse. The features are dusky, the eyes are hazy, the lips have a venous color, the respirations are short and husky; moist, wheezy murmurs are heard everywhere in the chest; the pulse is sometimes irregular, with some degree of fulness. In some cases it is small and sharp, while the heart-sounds are altered or modified by the presence or absence of atheromatous disease of the aorta, which is a frequent complication in these cases. Bleeding from the arm has fallen into almost absolute disuse. Forty years ago, arm-bleeding was employed in the treatment of the majority of diseases; now it has lapsed into total neglect and oblivion. Doubtless as a remedial means it was abused, and the principle of its use by many either ignored or not understood; just as in modern times the use of alcohol in its many forms as a stimulant has been overdone, and the principle on which it should be employed overlooked.

In the disease now under consideration, not only is there obstruction to the circulation through the liver by the morbid changes which its parenchyma has undergone, an obstruction which is the parent of the abdominal dropsy, but there are superadded pulmonary engorgement, difficult breathing, an increasing venosity, and consequently a more diffuse dropsy; serous effusion infiltrating the lower extremities. The heart's action is often irregular and labored: it is attempting to drive the blood, already vitiated by bile-products, through the pulmonary capillaries, which from the accumulating impurities still further resist its passage. In such circumstances it is in vain to expect relief from powerful hydragogue purgatives alone. A few ounces of blood taken from the arm will secure an amelioration of the symptoms which no other measure produces. It is like, even equivalent to, lightening the load which an overtaxed horse has to draw. The relief is most marked: the heart's action becomes regular and steady, the breathing less labored; nor is this the only advantage—remedies which in the overcharged plethora of the venous system were inoperative, now are capable of exciting the organs to activity. Small doses of mercury, squills, and digitalis, will now induce a diuretic action. The urine increases in quantity, the proportion of water increases, its specific gravity diminishes, it is sufficiently dilute to keep the urates in solution, and the thick, muddy, scanty urine is replaced by a clear and more healthy-looking excretion. The abdominal walls become less tense, the anasarca of the lower extremities decreases, and there is a general, but for the most part only temporary, improvement, in the condition of the patient. The improvement, however, is sufficiently marked to justify a steady perseverance in diuretic agents. The iodide of potassium and cream of tartar, the acetate of potash and digitalis, with sweet spirits of nitre, the infusion of broom-tops, juniper-infusion, with squills, and many other combinations, may be given with advantage. The dropsy may by these means be kept down for a considerable period, for months, or even longer; a complete recovery, permanent and durable, must not be expected. The next and most formidable and altogether irremediable stage of the disorder is characterized by great distention of the abdominal walls, a distention which gives to the skin a shining and glistening appearance. The slightest touch by percussion elicits the most marked impulse to the opposite fingers. Emaciation during the preceding weeks has generally been most marked. The attenuation of the abdominal walls, distended to the utmost by the fluid accumulations in the belly, has rendered visible the course of the external abdominal veins. Among these the course of the external epigastrics and their inosculation with the external mammary veins on both sides become palpably evident. These superficial veins in many cases acquire a size proportionate to the extent of the obstruction to the portal circulation through the liver. It is a circuitous route pursued by the blood in its return from the hæmorrhoidal and other veins of that series, which makes for itself a passage through this indirect channel to the external mammary veins, and thus reaches the right side of the heart. It is at this point that the dropsy acquires perhaps its maximum degree; not only are the abdominal walls tense, but the anasarca of the lower extremities is on the increase. The urine, which is usually scanty, high-colored, loaded with urates having a pinkish tinge, becomes now slightly albuminous, a condition dependent on the retarded circulation through the kidneys—the outpour of blood from the venous plexus and the emulgent vessels being obstructed by the tardy flow of blood through the inferior cava; and, if, to all this, pulmonary engorgement be added, the case of the patient is most unpromising. Hydragogue purgatives, particularly the podophyllin, with the subchloride of mercury, will produce temporary relief. If, however, there be an evident decrease of the tropsical tension, an effort may be made to induce a greater outflow of urine—it rarely succeeds; nevertheless, it is the direction in which remedies must work. A few days will indicate the prospect of further relief by medicine; and if the fluid remains undiminished, the urine still scanty, the breathing oppressed, and the lower extremities still more anasarcous, the loose tissue of the scrotum and penis being infiltrated with fluid, the time has arrived, and should not longer be delayed, of obtaining relief by tapping. It frequently happens that the reduced tension following the drawing-off the large quantity of serum from the cavity of the belly quickly sets free the excretory functions of the kidneys, and within twenty-four or forty-eight hours the urine is doubled in quantity, the albumen slowly disappearing from it, and the urates, no longer tinted with the oxidized products of uric acid, are dissolved in the augmented volume of urine.

Diuretics should now be chiefly relied on. To the more active of those already mentioned may, where the circumstances of the patient permit, be added those wines which are specially of a diuretic character. There are many of the Hock and Rhine wines which eminently possess this property; some also of the Hungarian white wines are actively diuretic. In hospital treatment there are certain spirits which may be selected with a similar object. Gin added to a cold solution of cream of tartar; gin and the sweet spirits of nitre; gin to which a few grains, at most ten, of the nitrate of potash have been added, are cheap and active diuretics. Hollands, which is a corn-spirit strongly impregnated with juniper, when properly diluted, is also an appropriate diuretic. The spiritus armoraciæ compositus of the Pharmacopœia has with some a powerful diuretic action. Such is a general outline of the principles upon which the treatment of hepatic dropsy should be conducted. Each case will probably exhibit some complication or variation peculiar to itself. The unfavorable and rapid progress of some cases in comparison with others is most remarkable. Such cases are generally distinguished by severe gastric symptoms at the earliest stage; the morning retching being oftentimes supplemented by hæmatemesis, severe gastralgia, and from time to time a marked icteritic stain of the conjunctivæ. In the commencement, such symptoms are best relieved by leeches to the epigastrium, or by a succession of blisters to the same region; the internal remedies being mercurial purgatives; and when the stomach has regained its power of retaining ingesta, the hydrochlorate of ammonia, or, as it is now named, the chloride of ammonium, in ten-grain doses, with taraxacum extract, may be given, and made more palatable by the addition of a few drops of the dilute hydrochloric acid, and with a little syrup of ginger, which takes away the disagreeable urinous taste of the salt. As a purgative, if a continuance of mercurials be not thought desirable, the compound gamboge-pill will be found very serviceable.

The prognosis of most cases of hepatic dropsy, if connected with or arising from the abuse of stimulants, must be unfavorable. Ultimate recovery in no case can be expected. Although the disease may for a time be retarded in its progress, and the fatal termination protracted, yet a permanent restoration to health, or the obliteration of the organic change in the hepatic structures, pathology forbids us to hope.

2.—*Nitrite of Amyl in Epilepsy.* By S. Weir Mitchell, M. D. [Philadelphia Medical Times, April 15, 1872.]

I have long desired to make use of nitrite of amyl at the outset of a case of epilepsy, but only of late have been so fortunate as to have charge of suitable cases. A case suited to make practical the exhibition of this rapid agent should have some distinct aura or other form of warning symptom, and this must be of such a nature as to leave the patient long enough in possession of himself to enable him to employ the remedy.

A few weeks ago, a young farmer consulted me as to a peculiar form of epilepsy, which seemed most suitable for testing the value of various modes of cutting off the attack by interfering with the early symptoms.

J. C., aged twenty-three, was excessively prone to self-abuse until, at the age of eighteen, he began to indulge to an extravagant extent in venery. At twenty he had a chancre, but thus far no secondary results. On the 4th of March, 1871, he became slightly intoxicated, went to a neighboring city, and for some days gave himself up to sexual excitements to such an extent, as he describes it to me, as to show that either he had unusual virile power, or that he exaggerated his prowess. Be this as it may, there is no doubt that he ran into great excess, and that the punishment was distinctly born of the offence. On March 9th, he felt feeble, but nevertheless had connection four times that night. On the 10th he had, twice, twitching of the left forefinger. On the 11th this grew worse, and on the 12th, after unusual sexual excesses, he had a prolonged spasm of the left arm. It began in the hand, and in a few minutes involved all the fingers in forced flexion. Then the wrist bent, and the flexed forearm was forcibly contracted. The attack, which surprised the patient but did not alarm him, ended with slight vertigo. A week went by without further trouble, when a series of complete epileptic fits began, always preceded by the local convulsion which I have described. The fits, which at first came on daily, soon lessened in number, and of late have recurred but once a week. On two occasions they have been sudden and without warning, but in all other cases the hand has been affected with spasm, followed in a few minutes by vertigo, twisting of the head to the left side, left unilateral convulsion, and finally bilateral spasms, with occasional tongue- or cheek-biting. The attack is followed by prolonged stupor.

Many remedies have been vainly employed, and even the bromides in full doses fail to do more than lessen the number of attacks; while strychnia, valerianate of quinia, zinc, and other agents have all alike failed to afford relief.

I made many experiments with a view to cutting short the fit by interfering with the precedent local spasm; but neither blisters along the nerve-tracks which are not tender, nor a ligature tightly applied, proved of any use. I then gave the patient a drachm of chloroform in a phial, directing him how to inhale it from a handkerchief, but soon found that he was unable to inhale enough of it to serve his purpose. As a last resort, I gave him in a very small phial three or four drops of nitrite of amyl, and showed him how to inhale it by putting the open phial up one nostril while with one finger he closed the other, and then made a few full inspirations. The first attempt failed, because, as he said, the spasm of the left limb made him nervous. On the second occasion he began to breathe it the instant the fingers twitched—having pulled the cork of the phial with his teeth. In a few moments he felt his face flush, the carotids beat violently, his head felt full, and, the spasm ceasing, the attack at once, and for the first time in his experience, was cut short. Four days later he thus cut short another attack; and the experiment has since succeeded in eleven fits, and failed, from too late use of the nitrite, in two. Moreover, the attacks have lessened in frequency, and now come on only once in ten to twenty days. Not only is there no evil effect from the drug, but his memory has improved, and his nutrition gained considerably. He is again taking bromide of lithium.

In a second instance of epilepsy, with aura proceeding from the right hand, the nitrite of amyl has been successfully used in two attacks; but commonly the cerebral phenomena in this, as in most cases, follow too soon to be of value—or, rather, too soon to allow of time for the nitrite to affect the intracranial circulation.

This must, unhappily, be the case in most epilepsies; but, in all which have an aura or local spasm distant enough in time from the cerebral symptoms, it may be of value, and ought certainly to be employed.

I hesitate, as yet, to speculate on the relations between the physiological influence of this interesting agent, and the present views of the mechanism of epilepsy; but I do not feel justified in withholding any knowledge, however limited be the foundation on which it rests, when it has proved so distinctly valuable.

In another class of rare cases it may also prove of service. There are certain epilepsies in which the spasms last for hours—one fit following another. In these I commonly employ with success injections under the skin of bromide of lithium, using thirty or forty grains in three or four localities; but I have twice checked these attacks at once by inhalations of the nitrite of amyl. In one of them there was a second fit, but no more—which was unusual. In the other, which lasts always several hours, I used the nitrite at the close of an hour, in the third convulsion. Relaxation instantly occurred; the fit passed off, and no other followed. Ether has been frequently employed in this case; but it merely mitigates the attack, and its use has to be kept up for hours.

OBSTETRICS AND DISEASES OF WOMEN.

1.—*Rupture of the Membranes before the Completion of Pregnancy.* By J. E. Burton, L. R. C. P., London. [British Medical Journal, March 9, 1871.]

Mauriceau, in his observations, has recorded sixteen cases, in which a largish discharge of a watery fluid occurred in the later months of pregnancy, at periods varying from seven days to two or three months before the setting in of labor. In the earlier observations, he gives it as his opinion that the discharge comes from *quelque espèce d'hydropisie de matrice*, for the reasons that when labor set in the patient again discharged a considerable quantity—*ces dernières eaux ayant percé comme à l'ordinaire;* and that, if they had been the *véritables eaux de l'enfant*, labor would have set in a short time after their escape. In his later observations, however, he inclines to the opinion that the discharge was *les véritables eaux de l'enfant* which had escaped from a small rupture in the membranes high up, in such a position that the rupture did not correspond to the internal orifice of the uterus, so that the waters did not entirely escape; and, a good part remaining, labor did not set in for a long time after the first escape of the waters, which renewed themselves in the interval. This change of opinion, for which he gives no reason, is all the more singular from his saying in one place that it is impossible for the ruptured parts ever to reunite. Dr. Carl R. Braun, of Vienna, in his "*Lehrbuch der Geburtshülfe*," pp. 184, 185, among other kinds of dropsy of the womb, mentions two which occur during pregnancy, and are characterized by discharges of water before its completion. The first he calls *hydrometra ascitica*, and defines it as a collection of fluid between the chorion and uterine walls. He says further of this, that the discharge of this fluid does not interrupt the course of pregnancy, and that the state can be recognized after withdrawal of the after-birth. The second he calls *Hydrallaute*—i. e., "*das falsche Fruchtwasser*," or a collection of fluid between the chorion and amnion. He says of this, that the discharge of the fluid under this condition often interrupts the course of pregnancy, and that it can duly be recognized with precision after the withdrawal of the after-birth, by the separation of the amnion from the chorion. My own experience is at present not very extensive, but I should incline to the opinion expressed by Dr. Radford, that the discharge is never real liquor amnii; and further, that it is always from some dropsical condition of the membranes external to the amnion.

2.—*Intermenstrual Dysmenorrhœa.* By Wm. O. Priestly, M. D. [Medical Times and Gazette.]

At a meeting of the Royal Medical and Chirurgical Society, Dr. Priestly read a paper on intermediate or inter-menstrual dysmenorrhœa. He pointed out that, although much had been written concerning dysmenorrhœa, and several forms of it had been described in accordance with the pathological views taken of its causes, the description of the several varieties was ordinarily limited to the time of the catamenial period, with the two or three days additional which may precede and follow the menstrual flow. From time to time, however, cases of a more obscure kind presented themselves, in which the chief suffering is remote from the actual menstrual period, but comes on, nevertheless, with the same punctuality, and is probably dependent on organic changes associated with the production of the catamenia. Probably other practitioners had observed like instances, as they were not unfrequent, but, as the author had met with no description of them, he brought the subject before the Society as a fragmentary contribution to the pathology of uterine affections, which might possibly evoke further elucidation by discussion. In all the cases detailed severe pain was experienced by the patients midway in the menstrual interval. The pain commonly came on about fourteen days after a catamenial period, and, after lasting a variable number of days, ceased before the supervention of the next expected period. In one case the pain, beginning midway in the interval, ran into the following monthly period, and was relieved by its flow. The suffering was constantly referred to one or other ovarian region, and in three cases out of four marked tumor, or thickening from old adhesions, was found in that locality. The reason for the occurrence of pain in the inter-menstrual period, and with such regularity, was not, in the present condition of our knowledge, perfectly obvious. A study of the physiological and pathological conditions left little doubt, however, that it was due to perturbations on the function of "spontaneous ovulation" habitually going on in the ovary. Hypertrophy of the structure of the ovary, or thickening of its indusium, would lead to undue vascular excitement, and impede the advance of ova to the surface in their attempts to attain maturity. It was not unreasonable to suppose, from all the known facts of the case, that preparation for an approaching period began in the ovary ten or fourteen days before the occurrence of the monthly uterine discharge, and, if the initial steps in the process of ovulation were opposed by certain pathological conditions, pain would ensue. Nay, in the absence of distinct organic change, it might readily be imagined how special irritability in the ovary would cause an unusual amount of disturbance whenever there was occasion for the exercise of fresh activity in the organ. This latter class of cases would partake more or less of a neuralgic character. The treatment would depend on the pathological condition as ascertained by examination. The pain being only a symptom, it would be needful to inquire into the cause; and, if there were tumor, or thickening depending on former inflammation, absorbent remedies would be indicated. If no organic change of structure could be detected, anti-neuralgic remedies, such as quinine, iron, and arsenic, would best answer the purpose of cure.

3.—*Caries of the Ossa Pubis following Delivery; Septicæmia; Death.* By R. Eardley Wilmot, M. R. C. S., L. R. C. P., Resident Accoucheur to King's College Hospital. [Lancet, March 16, 1872.]

The following case, illustrating an extremely rare puerperal complication, with fatal issue, and complete *post-mortem* examination, is, I think, of sufficient interest to deserve publication. The notes were very carefully taken by Mr. J. H. Philpot, clinical clerk.

Jane C——, aged twenty-three, was admitted into King's College ward on January 29, 1872, under the care of Dr. Priestley. She had been perfectly healthy up to December 1, 1871, on which day she was delivered, easily and naturally, of her first child. Convalescence seemed to progress favorably till the third week after delivery, when she began to suffer occasional pain in the hypogastric and left inguinal regions, which much increased on moving the left leg. At the same time an abscess formed within the labia, anteriorly, and, bursting spontaneously into the vagina, discharged much foul, creamy matter. This discharge continued throughout her illness. Marked constitutional disturbance now set in—rigors, loss of appetite, and rapid emaciation. To these symptoms a harassing night-cough and extensive bed-sore over the sacrum soon became added.

On admission, she was seen to be extremely emaciated and prostrate, suffering from pain and tenderness in the hypogastrium, aggravated by any attempt at motion, and from an extremely offensive discharge, thin, dark, and copious, which poured in gushes from the vagina on the slightest movement. The left leg was abducted and motionless, showing some slight redness and œdema over Scarpa's triangle. On vaginal examination, the uterus appeared healthy, and normally involuted; no orifice for the exit of discharge could be detected in any part of the vaginal tract. The lungs seemed healthy on examination; tongue red, glazed, and aphthous; the expression of the face extremely anxious; the breath had a remarkably sweet odor. Pulse 144, full, and non-resistant; temperature not much elevated, but showing evening exacerbations. Appetite fair; slept very badly. She was placed on generous diet, with six ounces of brandy per diem, and the following mixture was ordered to be taken three times a day: chlorate of potash, ten grains; dilute hydrochloric acid, ten minims; syrup of oranges, ten minims; infusion of chiretta, one ounce. Also twenty grains of chloral at bedtime. The bowels were relieved by enema. A vaginal injection, containing Condy's fluid, ordered twice daily.

From the time of her admission the disease made rapid progress. She grew weaker daily, and became more and more emaciated. The discharge increased in quantity, and continued horribly fetid. On the 4th of February the temperature rose to 102° Fahr., and she seemed in every way worse. An erythematous patch appeared on the left thigh, over Scarpa's triangle, which soon became the seat of a tense, painful swelling, in which fluctuation could be distinctly detected. On the 8th of February this swelling was freely opened by Mr. Cross, house-surgeon, and a large quantity of foul pus discharged. Slight temporary relief followed, but by the 13th she was again worse, and seemed to be sinking fast. Brandy increased first to eight ounces and then to ten ounces per diem. On the 15th she seemed quite collapsed; pulse 150, very weak; temperature 97°. Brandy raised to twelve ounces per diem. Evening temperature 105°. On February 16th she was weaker; temperature still 105°. She sank, and died at 1.30 A. M. on February 17th.

Autopsy Thirty-six Hours after Death.—Body extremely emaciated. Thorax: Adhesions in left pleura; lower lobe of left lung hepatized, the consolidated portion studded with numerous gray, softened points (metastatic infarctions), from which a purulent matter was obtained by scraping the surface. Heart healthy.—Abdomen: Liver large, smooth, and fatty; kidneys pale and fatty; uterus healthy, and well involuted. On making an incision through the skin covering the symphysis pubis, the following conditions were found: The ossa pubis were separated at the symphysis to the extent of half an inch, the interarticular cartilage and subpubic ligaments having almost entirely disappeared. The bones themselves had undergone extensive caries; they were black in color, rough on the surface, and soaked in fetid pus. From the symphysis pubis descended a large sac, partly filled with foul, flaky matter. Its cavity extended in two directions, first, downward under the mons veneris and labia to a point situated upon the anterior vaginal wall, just within the orifice, where it communicated with the vagina by a small aperture about three lines in diameter; secondly, outward and downward to the left groin, where a large, irregular cavity occupied Scarpa's triangle, opening externally by the incision made on February 8th. There were numerous burrowing sinuses around and between the muscles in all directions. The left acetabulum and head of the femur showed signs of commencing inflammatory mischief.

It will be seen that there was free exit for the pus from the diseased bone by two apertures—through the vagina, and through the artificial opening in the thigh.

The specimen is preserved in the King's College Museum.

The case seems to me full of interest; the diagnosis of this disease must always, I think, be difficult and doubtful, the prognosis unfavorable, and all treatment unavailing. I should be very glad of any suggestions as to its probable cause and pathology.

4.—*Intermenstrual or Intermediate Dysmenorrhœa.* By W. O. Priestley, M. D. [British Medical Journal.]

The author pointed out that, although much had been written concerning dysmenorrhœa, and several forms of it had been described in accordance with the pathological views taken of its causes, the description of the several varieties was ordinarily limited to the time of the catamenial period, with the two or three days additional which might precede and follow the menstrual flow. From time to time, however, cases of a more obscure kind presented themselves, in which the chief suffering was remote from the actual menstrual period, but came on, nevertheless, with the same punctuality, and was probably dependent on organic changes associated with the production of the catamenia. Probably other practitioners had observed like instances, as they were not unfrequent, but the author had met with no description of them.

In all the cases detailed, severe pain was experienced by the patients midway in the menstrual interval. The pain commonly came on about fourteen days after a catamenial period, and, after lasting a variable number of days, ceased before the supervention of the next expected period. In one case, the pain, beginning midway in the interval, ran into the following monthly period, and was relieved by its flow. The suffering was constantly referred to one or other ovarian region, and, in three cases out of four, marked tumor, or thickening from old adhesions, was found in that locality.

The reason for the occurrence of pain in the intermenstrual period, and with such regularity, was not, in the present condition of our knowledge, perfectly obvious. A study of the physiological and pathological conditions left little doubt, however, that it was due to perturbations in the function of "spontaneous ovulation," habitually going on in the ovary. Hypertrophy of the structure of the ovary, or thickening of its indusium, would lead to undue vascular excitement, and impede the advance of ova to the surface in their attempts to attain maturity. It was not unreasonable to suppose, from all known facts of the case, that preparation for an approaching period began in the ovary ten or fourteen days before the occurrence of the monthly uterine discharge; and, if the initial steps in the process of ovulation were opposed by certain pathological conditions, pain would ensue. Nay, in the absence of distinct organic change, it might readily be imagined how special irritability in the ovary would cause an unusual amount of disturbance whenever there was occasion for the exercise of fresh activity in the organ. This latter class of cases would partake more or less of a neuralgic character.

The treatment would depend on the pathological condition as ascertained by examination. The pain being only a symptom, it would be needful to inquire into the cause; and if there were tumor, or thickening depending on former inflammation, absorbent remedies would be indicated. If no organic change of structure could be detected, antineuralgic remedies, such as quinine, iron, and arsenic, would best answer the purpose of cure.

5.—*Impregnation without Rupture of Hymen.* By C. R. Francis, M. B., Deputy Inspector-General of Hospitals. [Indian Medical Gazette, December 1, 1871.]

A few years ago, when officiating as civil surgeon in one of thestations of the northwest provinces, I was asked to prescribe for a lady whose prevailing and prominent symptom was *sickness.* It was unceasing. Exaggerated in the morning, it continued throughout the day. At night only, when my patient slept, was relief experienced. She had been married a few months. I pronounced her to be pregnant. "I can't believe it," said the husband. "Impossible," said the wife. "Why?" I asked, "can any thing be more likely?" (The lady was twenty-eight years of age, well nourished and in good condition; and the husband was young and vigorous.) I founded my diagnosis upon the condition of the nipple and the circle surrounding it, in connection, of course, with other circumstances. Sufficient time had not elapsed to allow of my ascertaining any thing satisfactory from any kind of *examination.* The couple were quite satisfied that they were right, and that I was wrong. A *dhaee* had told the lady so. I was but a youngster in those days; and the snow had not appeared upon the top of the mountains. It was, therefore, a delicate subject for me to talk about, so I took "Gooch on some of the Diseases peculiar to Women" out of my library, and, marking down the passage where the dissecting-room scene is described in which John Hunter declared that a fœtus would be found in the uterus of an apparent virgin, although an inquisitive student had announced the existence of a hymen—(Hunter had based *his* diagnosis upon the state of the breasts), sent the book to the husband, and asked him if that was not the condition under which he believed his wife to have remained unimpregnated, viz., *the existence of an unruptured hymen.* His reply was in the affirmative. Shortly afterward, upon my recommendation, the lady was sent for change of air to a neighboring station; and eventually she went to the hills, where, at the end of the ninth month, she was duly confined. I believe that the hymen remained unruptured to the last, as the medical officer who attended her wrote to me to say that there was some kind of membranous impediment to the birth, but which finally gave way as the head advanced. Such cases are not common, and should, therefore, be recorded when they occur.

SURGERY.

1.—*Tumor of the Head of the Humerus.* By Thomas Annandale, F. R. S. E. Abstract of a Clinical Lecture. [British Medical Journal, March 9, 1872.]

I wish to-day to direct your attention again to the case of Master C., aged eleven, who has lately been under treatment in the wards. The patient's father told us, on inquiry, that a swelling was for the first time observed two month's ago on his son's left shoulder. The swelling gave rise to no inconvenience, but has slightly increased in size since first noticed.

A careful examination of the swelling, made before you on a former occasion, taught us that it was a growth from the head of the humerus, that it was of the size of a pigeon's-egg, hard to the touch, firmly connected to the bone, and completely covered by the deltoid muscle and tissues superficial to this muscle, which with the other soft textures was freely movable over the tumor. A further examination of the deep connection of the growth showed that the long axis of its neck ran in the direction of the outer margin of the bicipital groove, and passed upward to a little below the anatomical neck of the bone. The tumor itself seemed slightly to overlap its neck, but owing to the covering of the thick muscle this point could not be very accurately determined. The movements of the joint and arm were perfect.

You will remember that, after this examination, I remarked that, having concluded that the case was one of tumor growing from the bone, our next duty was to determine, if possible, the nature of the growth, in order that the proper treatment for it might be considered. The diagnosis to which we came was, that the growth was either an exostosis or a firm cartilaginous tumor, but that it was not easy to determine between these, owing to the thick covering of the deltoid muscle. On this occasion I further remarked that, if the case were one of exostosis, the proper treatment would be to expose the neck of the growth, taking care to avoid any injury to the capsular ligament of the shoulder-joint, and saw through it close to its connection with the bone. (The pathology of a cancellated exostosis was then described in connection with this treatment.) If, however, the tumor should prove to be cartilaginous, a more severe operation would be required, because the pathology of these latter growths teaches us that their structure is almost always infiltrated into the substance of the neighboring bone, as illustrated in these specimens (preparations shown). A cartilaginous tumor in this situation might be removed in one of three ways: 1. By enucleation of the tumor; 2. By excision of the head of the humerus, together with the tumor; 3. By amputation at the shoulder-joint. The first of these I did not approve of, as it was a most uncertain method of getting rid of the whole growth; and it could not in this case be carried out without injury to the joint, and therefore it had no advantage over the second plan, which, from my experience of two similar cases in the practice of the late Mr. Syme, was also somewhat uncertain, unless the shaft of the bone where divided was perfectly free from infiltration. The third plan was not at present to be thought of, as the disease was too limited, and too little troublesome to necessitate so serious an operation. It might here be asked, Why interfere with the tumor if it be not causing inconvenience? The reply to this is, that it is a sound principle, with few exceptions, to remove all morbid growths at as early a period of their growth as possible, provided this can be done without danger to life. My proposal, therefore, is to expose the tumor and its connections by means of a free incision carried down through and parallel to the fibres of the deltoid muscle. Should it prove to be an exostosis, its neck will be divided and the growth thus removed; but, if the tumor be cartilaginous and infiltrate the surrounding bone, I will excise it together with the head and neck of the bone, carefully examine the shaft of the bone where sawn through, and, if it show any traces of disease, remove another slice, so as to take away, if possible, all the disease.

The operation, gentlemen, you saw; and, as the growth was a well-marked example of the cancellated exostosis, it was removed alone, without injury to the joint, by dividing its neck, which was nearly two inches in length and an inch in width, and entirely osseous in its structure. The tumor had a thick layer of cartilage on its surface, and was surrounded by a distinct capsule formed of the expanded periosteum. After the operation, the whole surface of the wound was freely touched with Mr. De Morgan's solution of chloride of zinc, and Mr. Lister's antiseptic dressing applied. The result, as you see (patient again shown), has been very satisfactory; the wound is soundly healed, and the patient appears to have suffered little either locally or constitutionally from the operation.

2.—*Spontaneous Rupture of the Spleen.* By Surgeon T. ODEVAINE, Lahore, India. [Indian Medical Gazette, December 1, 1871.]

The subject of the above, the wife of one of the Sikh cooks attached to the regiment, had been with her sister grinding corn on a small hand-mill, or *chukkee*, in a hut close to the lines. When she had finished, she was going toward the married quarters, and, on passing a well, she complained of feeling giddy, sat down and drank a little water, then went in company with her sister to the hut of a married Sepoy, which was a few yards from the well; here, on reaching the threshold, she sat down, and again complained of a feeling of giddiness, and immediately lay down. On her husband being called, and trying to raise her, she once more lay down, and within five or six minutes after died. A court of inquest was convened to inquire into the cause of this woman's death. I made a *post-mortem* examination about sixteen hours after death. The body was that of a young woman, about twenty-two years of age, tolerably well nourished, and presenting no external marks of violence. The abdomen appeared prominent and tense; on opening its cavity, the omentum was found discolored by blood, and several pints of serum and clotted blood were found extravasated within the peritonæum. I at once suspected rupture of the spleen, and carefully removed this organ. It was about four times its normal size, not remarkably friable, and presented a superficial rent on the middle of its inner aspect, about two and a half inches long, and running parallel to its length. The liver was pale and much larger than in health; the heart covered with a good deal of fat for so young a subject, but otherwise healthy; uterus empty.

The rupture of the spleen was first attributed to violence, but all the evidence proved that she had received no blow of any kind, and her sister had been with her the whole time, from the moment they went to grind their corn till her death. I suggested that she might have accidentally hit her side in turning the handle of the *chukkee*, but the base of this was so wide as to preclude the possibility of such an occurrence.

The deceased had been subject to epilepsy and ague, but had not had a fit for many days before her death.

I believe the above to have been a case of spontaneous rupture of the spleen, and in this country, where a comparatively slight blow over this organ has frequently caused its rupture, it becomes of interest to know that this occurrence may take place spontaneously.

It will be observed that the rent was on the concave surface or *hilus* of the spleen, the site of entrance and exit of its numerous very large vessels, and, though not deep, probably as occupying its most vascular part, accounted for the rapid death of this woman from the time of her first complaining of giddiness, which could not have been more than twenty minutes. In the cases of ruptured spleen caused by violence, which have come under my notice, I have always found the injury at the external surface or anterior edge of the organ, whereas, in the present instance of spontaneous rupture, the rent was at its *hilus*. Perhaps this may be of some diagnostic use in medico-legal inquiries of a similar nature.

3.—*Gangrene of Foot from a Bunion.* Clinic of Prof. S. D. Gross. Reported by Mr. F. Woodberry. [Philadelphia Medical and Surgical Reporter, May 11, 1872.]

This man, aged fifty-six years, showed a tumefied foot from which the great-toe had sloughed away, leaving its phalanges and metatarsal bone protruding from an ulcerated surface. This extended over the dorsum from the sole to the instep, and from the last metatarsal bone to the plantar surface of the internal aspect of the foot. The bottom of the ulcer was foul with granulations, and several patches of gray cacoplastic lymph, and discharged a sanious, offensive secretion.

This trouble commenced some years previous to his appearance at the clinic (April 10th), from a bunion over the metatarso-phalangeal joint, caused by the chafing and pressure of a shoe. This finally became the seat of inflammation; mortification followed, producing sphacelous with necrosis of the phalanges and metatarsal bone, which are dead, and project from the surface, denuded of their periosteum.

Prof. Gross said that amputation by Chopart's operation would perhaps be the proper treatment here, and may eventually be necessary, but conservative surgery requires us to take away no more tissue than is absolutely necessary, and the more of the foot we can save, the more useful it will be to the patient. [After administering chloroform, two incisions were made, commencing at an acute angle about the middle of the dorsal surface of the metatarsal bone, and carried forward and united on the under surface. Then the soft parts were retracted and the bone divided with the bone-forceps. It was found to be about an inch in thickness and abnormally cancellated, softened and vascular from the effects of the inflammation.] The sesamoid bones should always be removed with part of the flexor tendon, as they interfere with future usefulness and repair. The great-toe should never be amputated at the metatarso-phalangeal articulation, as the large head of the metatarsal bone would make an awkward and inconvenient prominence, seriously interfering with the boot or shoe.

4.—*Cyanosis; Murmur with the First Sound of the Heart: Patent Foramen Ovale.* By Edward Mackey, M. D. [British Medical Journal.]

A female child, five months old, was brought as an out-patient to the Children's Hospital on October 23d. Its face and extremities were of dusky color; it had cough, hurried breathing, and much palpitation, and a distinct murmur with the first sound over the heart. I did not ascertain whether this was propagated or not up the great vessels. The child had been dark-colored from birth, and had had convulsions at intervals. It did not seem colder than others. The mother was not strong. She had suffered during pregnancy from palpitation and faintings, and at the seventh month had fallen down-stairs. The child was born at the full time. She had two other children, healthy; and she had no cardiac disease. The child was ordered an expectorant and stimulant mixture, and I saw it once more, and verified the presence of murmur. A few days afterward it died in convulsions. On the following day I made a *post-mortem* examination, at which Dr. Foster was present. The fontanelle was not closed. The abdomen was distended. The chest was markedly cone-shaped, the apex being at the neck. The surface was not so dark as during life. On opening the thorax, the lungs were in part pink-colored and healthy; in part dark and collapsed. The pericardium contained more than the usual quantity of serum. The heart was larger than normal, but in its ordinary position. The right cavities were larger than the left, and contained clots; the left were empty. The walls of the right ventricle were somewhat hypertrophied. The aortic and pulmonic valves were healthy. We examined carefully the orifice of the pulmonary artery, and there was no contraction about it; on the contrary, it was, if any thing, larger than usual; it was so far larger than the orifice of the aorta that it admitted my little finger readily to the second joint, while the aorta admitted the same only to the first joint. The foramen ovale was patent, admitting a goose-quill above the edge of its imperfect valve. The Eustachian valve was well developed. The liver was large.

The interest of this case lies specially in the fact of a murmur being heard without there existing any other condition to account for it than the open foramen. There was no contraction nor abnormality of the pulmonary artery; and thus the case supports the views of Dr. Markham, and those expressed by Dr. Foster in his paper on the subject in the *Dublin Quarterly Journal of Medical Science* for August, 1863.

5.—*Myxomatous Cyst; Ligature of the Tumor.* By A. S. G. Jayakar, M. D., Ahmedabad. [Medical Times and Gazette.]

M., a healthy-looking infant of about eight months, was admitted into the dispensary attached to this hospital on March 2, 1871, with a cystic-looking growth situated about half an inch behind the anus, and just over the lower end of the coccyx. It was about the size of a medium-sized apple, firm, and rather hard above, and soft and cystic in its lower part. It was first observed as a small growth a few days after the child's birth, and then kept on increasing in size. There was some amount of pain on squeezing the tumor; but it was, indeed, so slight that its connection with the spinal cord may have been fairly disputed. On exploring the lower part of the tumor with a grooved needle, a viscid, transparent, mucus like substance escaped; it was sticky in its feel, and heat seemed to have no effect on it. The tumor was tapped, and about four ounces of the same sticky, viscid fluid removed. When the fluid contents were removed, the nature of the harder and firmer part of the tumor rendered the diagnosis of the case easy. The lower bagging portion of the tumor soon filled up again, and she was admitted into the hospital as an in-door patient on May 22d. The tumor having rather an undefined base, no active surgical interference was thought advisable, and a thick, strong ligature was therefore passed through it, and tied on either side. Within twenty-four hours, the ligature seeming to give rise to convulsions, was loosened, and again an attempt was made gradually to tighten it, so that by the end of three weeks the tumor had mostly separated, and a few touches of the knife completed its final removal from the body, leaving a red granulating surface to heal gradually. The wound has now mostly healed up; there is still, however, a patch of the size of a shilling, but time and stimulant applications may fairly be expected to complete the cure.

Cases of myxomatous tumors are still very few on record—either owing to rarity of such growths, or their being easily mistaken for colloid and similar tumors of a malignant nature. The physical condition and general structure may often lead to a wrong diagnosis; but the total absence of any signs of malignancy, together with a microscopical examination of the contents of the growth, would soon decide the question as to its nature. The upper part of this tumor, or rather the undefined pedicle described in the case, was solid; and, on making a section, the same glairy, viscid fluid as in the lower part oozed out. Under the microscope, it was found to be principally composed of fibro-cellular tissue, with fat-cells and oil-globules scattered in places, showing that already degenerative changes had commenced. The lower or cystic portion of the growth was divided into small compartments by bands of fibro-cellular tissue, extending in all directions, with vessels ramifying over the greater part of its inner surface. The viscid, glairy fluid under the micoscrope presented numerous large rounded cells (some were oval), with thready branches shooting forth from all sides of the cells, and freely anastomosing with each other.

6.—*Rupture of Aorta within the Pericardium.* By Benjamin Browning, M. D. [British Medical Journal, December 9, 1871.]

At a recent *post-mortem* examination, I obtained the morbid specimen now exhibited; and, as it appears to me of sufficient pathological interest to warrant my bringing it under your observation, I must beg your indulgence for a few moments while I narrate its history. It is a well-defined instance of rupture of the aorta within the pericardium. It was procured from the body of a blacksmith, aged thirty-four, who for the last twelve months had been constantly under my notice while at work in his forge, and who, during the whole of that time, was distinguished for energy and skill in his business. About fifteen months before his death, he consulted Dr. Kersey for palpitation and dyspepsia, and was also seen by me; but, in spite of frequent and careful auscultation, no morbid cardiac sounds could ever be detected, and a little palliative treatment speedily put him to rights. Since then, if questioned about his health, he always affirmed that he was quite well, except on one occasion a few weeks since, when he complained to me of indigestion, but would not take any thing for it.

On the evening of his decease, he worked hard at finishing a job up to seven o'clock, and was in his usual good spirits. He then made a hearty supper, and retired to rest. In less than half an hour, he suddenly exclaimed that he felt choking, and immediately expired. Being soon on the spot, I was able to gather these particulars with precision. A necropsy was made twelve hours subsequently. The body was that of a well-nourished man, of medium height and muscular development. All the thoracic and abdominal contents appeared normal, with the exception of the pericardium, which was much distended, and contained more than two pints of blood separated into liquor sanguinis and crassamentum, the latter very firm and unbroken. The source of this hæmorrhage was a small rent of the ascending portion of the aortic circle on the left side, not larger than a crow-quill. No distinct aneurismal pouch could be found; the rupture, therefore, appears to have been due to the giving way of a small atheromatous spot in the artery.

7.—*Simultaneous Dislocation of both Ends of the Clavicle.* By Stanley Haynes, M. D. [British Medical Journal.]

A girl, aged thirteen, rapidly growing, of lax tissues, and of a consumptive family, but who had always had good health, while washing the back of her neck with her left hand one morning in September, felt something give way in the shoulder of the same side. I found dislocation forward of the sternal end of the clavicle and partial luxation upward of the acromial one. There was very little pain. Both extremities of the bone were easily replaced by drawing the shoulder backward and downward, but the double deformity was reproduced immediately the shoulder was liberated. A pad was applied under a figure-of-8 bandage over the sternal end, and the arm was placed in a sling as a temporary measure. To a strap, fastening round the chest, a strap bearing a truss-pad was attached in such a manner that the pad kept the sternal end of the clavicle reduced, the other end of the strap passing over the shoulder, and diagonally across the back to the horizontal strap: the wearing of a sling kept the acromial end in its natural position. The patient soon afterward returned to school at a distance: she is now at home, and I have found the sling has been discontinued some time, and the straps have stretched and are useless, and that the ends of the bone are as mobile as, but not more than they were, when I first saw the patient, but that the sternal end does not become luxated unless the arm is raised, when it nearly always starts forward. I have modified a hernia-truss, so that the anterior pad keeps the sternal luxation reduced in all positions of the arm, while the band passes under the left axilla and the other pad lies between the right scapula and the spine; but this slips out of place. If any reader of this note will suggest a better (non-operative) treatment, I shall feel much indebted. Mr. Erichsen has kindly written me: "There are three cases on record of simultaneous dislocation of both ends of the clavicle."

Miscellaneous and Scientific Notes.

Injections of Ergot in Maladies of the Uterus. [Gazetta Med. Ital. Lombard., 1872, No. 4.]—Dr. Swideski, in a recent memoir, collects about forty cases in which subcutaneous injections of ergot were employed in various diseases of the womb, especially chronic metritis, in some displacements and in metrorrhagia; its action was in every case prompt and certain. The author used at first the solution of Beaujeau, and observes that, as the solution contains a larger quantity of alcohol, it acts more promptly, but excites greater pain, and frequently produces ulcers. The solutions employed are:

1.	Extr. aqu. secal. cornut.,	gram.	2
	Spir. vin. rectif.,		
	Glycerina, āā,	"	7
2.	Extr. aqu. secal. cor.,	gram.	2
	Spt. vin. rectif.,	"	5
	Glycerina,	"	12
3.	Extr. aqu. secal cor.,		
	Spt. vin. rectif., āā,	gram.	2
	Glycerina,	"	12
4.	Extr. aqu. secal. cor.,	gram.	1
	Spt. vin. rectif.,	"	1
	Glycerina,	"	8
	Aq. destil.,	"	4

In cases of chronic ulceration he employs solutions 3 and 4; where prompt action is needed, Nos. 1 and 2; the former

(Nos. 3 and 4) act in from one to two hours, the latter excite painful contractions in about half an hour. In some of the deviations of the womb the author reports the favorable results of the use of subcutaneous injections of ergot; he observes, however, that it is of no benefit if the case is of too long standing, if the uterus presents considerable inflammation, or adheres to other organs in the vicinity. In chronic metritis the injections are employed every two or three days, and, even where a complete cure cannot be hoped for, the leucorrhœa speedily ceases, and the uterus returns to its place.

The Treatment of Varices by Subcutaneous Injections of Ergotin. [Berlin Klin. Woch., 1872, No. 10.]—Langenbeck's recommendation of ergotin in the treatment of spontaneous aneurism led Vogt to test the action of the remedy in varicosities of the veins of the leg. At first, in a man sixty years old, who had suffered with varicose veins in the left leg, an entire syringeful of the solution (extr. sec. cor. aquos 2, sp. vin., glycerine, āā 7.5) was injected (0.12 ergotin) in the region of the central end of a varix six cm. long, and of the thickness of the little finger, every second day. In the course of eight days the varix had disappeared, and had not returned after six weeks, during which time the patient went about as usual. Besides this case, the author was enabled to prove the extraordinary action of the remedy in several patients in the chirurgical clinic, even in colossal ecstasiæ. At the author's suggestion, Dr. Potel undertook experiments upon animals, and succeeded in demonstrating, microscopically and sphygmographically, the action of the remedy upon the muscular element of arterial and venous vessels. Vogt, therefore, assumes that ergotin acts in the disease in question partly on the veins, and partly also by constricting the arteries, thus lessening the flow of blood to the veins. Compression of the varix by the slight inflammatory induration which forms at the point of injection can hardly come into question, since the more peripheric ecstasiæ, which must immediately increase in size, contract in precisely the same degree as those nodules at which the injection is made. Finally, the author describes several experiments from which he deduces that the subcutaneously-injected ergotin does not effect an exclusively local action, but operates through the vaso-motor centres upon the vessels.

Appointments, Honors, etc.—Prof. Samuel G. Armor, of Brooklyn, has been honored by Franklin College, Ohio, with the degree of LL. D. Drs. C. E. Buckingham and W. L. Richardson, of Boston, have been elected members of the Obstetrical Society of London. In the College of Physicians and Surgeons of New York, Dr. James W. McLane has resigned the chair of Materia Medica and Therapeutics, and has been appointed Adjunct Professor of Obstetrics and Diseases of Women and Children. Charles F. Chandler, Ph. D., has been appointed Adjunct Professor of Chemistry, and Edward Curtis, M. D., Lecturer on Materia Medica and Therapeutics, in the same college. Dr. Verneuil, a well-known surgeon and academician, and Professor of Surgical Pathology at the School of Medicine, has been appointed Professor of Clinical Surgery in the place of Prof. Langier, deceased. The latter appointment is considered a promotion, as the Clinical Professorship of Medicine or Surgery is the highest attainable by the Faculty in Paris. The authorities of the Middlesex Hospital, London, have resolved to appoint a lecturer on Psychological Medicine. Dr. Stromeyer, the distinguished Hanoverian surgeon, has been lecturing in St. Thomas's Hospital, London, on his surgical experiences and recollections. At the recent *concours* for the election of seven Aggrégés or Assistant Professors of the Paris Faculty of Médicine, choice was made of MM. Hayem, Damaschino, Fernet, Lancereaux, Bergeron, Duguet, and Rigal. There were thirteen competitors, and some of the judges have been heard to say that this is the most remarkable *concours* which has taken place for a long time, so great and so equal was the merit of the candidates.

The Theory of Subcutaneous Injection.—(Ber. Nat. Med., Ver. Innsbruck, I. 2. Rundsc., 1871.)—Hausmann, like Nussbaum (1856), and Muehe (1863), has noticed a set of sudden peculiar symptoms after the injection of morphine. These were pain and redness of the face, convulsions of the muscles of the lower jaw, anxiety, a pounding, frequent pulse (130), asthma, clonic spasms in both extremities, lasting five minutes. The convulsions first ceased, then the pulse became quieter, and finally a copious sweat broke out. An essential difference between the previously-observed cases and the latter consists in the fact that the former occurred in persons who had become accustomed by repeated use to the injections, and in this case it followed the first injection. Nussbaum's assumption, that in such cases the morphine has penetrated directly into the veins, is supported, chiefly, by the fact that the skin has been thickened by repeated injections and the veins enlarged, although this, as already mentioned, was not the case in Hausmann's patient, nor did any copious bleeding follow as in the other cases; still the author hesitates to accept Nussbaum's theory. The phenomena are not absolutely dependent upon the quantity, for the author injected only four drops. (In the Magendie's solution employed by the author there is one drop of acetic acid, besides alcohol; in the usually-employed morphine solution this is absent.) Hausmann advises for the attack itself a second subcutaneous injection at another point.

An Honor from afar.—Prof. Lewis A. Sayre has received the following communication from the legation of his Majesty the King of Sweden and Norway, dated

WASHINGTON, D. C., *May* 23, 1872.

DEAR SIR: I have much pleasure in informing you that his Majesty the king, my august sovereign, has been graciously pleased to confer upon you the Knighthood of the Royal Order of the Wasa, in acknowledgment of the services rendered by you to the study of medical science in Sweden.

Enclosing herewith the letter-patent which has accordingly been made out for you, together with the decoration thereto appertaining, I beg you to accept my sincerest felicitations on account of this well-deserved distinction. Believe me, dear sir,

Yours very truly,

(Signed) P. STENERSEN.

Chloral-Hydrate in Spasmus Glottidis. [Jahrbuch für Kinderkrankheiten, 4. H., 1871.]—Dr. Rehn treated a case of spasmus glottidis, which had assumed a threatening character through the severity and frequency of the attacks, with chloral-hydrate. The attacks were perceptibly lessened and finally quieted. The child thus treated was seven months old. The medicine was well borne, and produced no disturbances of digestion.

Deaths from Snake-bite.—It is stated, says the *Homeward Mail*, that in the Tanjore district no fewer than nineteen persons in every 100,000 die annually from snake-bite. Taking the population according to the census, this would give nearly 330 deaths per annum in that district alone; and assuming the rate of mortality over the whole presidency of Madras to be only half that which prevails in the Tanjore district, and the population to be 30,000,000, we get the startling total of nearly 3,000 people annually dying from snake-bites.

Delicate Test for Gallic Acid.—Strasbourg (Pflüger's Archiv, IV.) announces an extremely delicate modification of Pettenkofer's test for gallic acid in the urine, suitable for clinical purposes. A strip of filtering-paper is dipped in urine, to which a little common sugar has been added, and on this paper, after drying, a drop of concentrated SO_3 is placed by means of a glass tube. If gallic acid is present, the paper assumes an intense violet color.

Boylston Prizes.—At the annual meeting of the Boylston Prize Committee, held June 3, 1872, a prize of $200 was awarded to J. Collins Warren, M.D., of Boston, for the best essay offered on the subject of "The Pathology of Malignant and Semi-malignant Diseases." A prize of $150 was also awarded for the best essay on the "Pathology and Treatment of Sunstroke," to Dr. Horatio C. Wood, Jr., of Philadelphia.

Proposed School of Medicine in Bordeaux.—It is reported (*Lancet*) that M. Jules Simon, the Minister of Public Instruction, has accepted in principle the creation of a Faculty of Medicine at Bordeaux to replace that of Strasbourg, and that a commission has been appointed to report upon the project in question. It is also in contemplation to establish a School of Medicine and a School of Pharmacy at Lyons.

The Chicago Relief Fund.—At a final meeting of the Committee of the Chicago Relief Fund, held at the house of Dr. Frank Hamilton, chairman, April 20, 1872, the treasurer, Dr. Hubbard, reported the receipt of the sum total of $6,292.91, which was disposed of as follows: Sent to Chicago, $5,934.51; sent to Wisconsin, $325; expenses of secretary, Dr. A. E. M. Purdy, $33.40.

West Virginia State Medical Society.—This Society met in Wheeling, June 5th, and continued in session for three days. Dr. R. H. Cummins, of Wheeling, was elected president for the ensuing year. The next meeting will be held at Parkersburg Mineral Springs, on the first Wednesday in June, 1873.

A Japanese Medical School.—The Government of Japan has undertaken to found an army medical school in that country, and three medical officers have been sent from Prussia to aid in the enterprise.

The New German University at Strasbourg.—In a recent number of the *Gazette Médicale*, Dr. Hergott furnishes an account of the new German university now being established in Strasbourg. He says that every possible effort will be made to attract first-class professors, who will be given to understand that Strasbourg will prove their surest road to arrive at Berlin, while their payment is to be very large. It is just stated that Prof. Gusserow, of Zurich, has accepted the chair of obstetrics at Strasbourg, and that his salary is to be 15,000 francs. This, in comparison with the salaries of the professors in Germany, is an enormous sum, their emoluments being derived from two sources—a moderate, fixed amount being contributed by the state, and the fees derived from the students. These last, for some of the professors, reach a large sum, amounting to more than 25,000 francs per annum, forming the recompense of their talent and industry, and for their zeal in the instruction of the students. This, says Dr. Hergott, is a just arrangement, for it seems to be contrary to all justice that a somnolent professor without auditors, and one who has the power of attracting crowds, should receive the same remuneration. Positive Germany has long since utilized this stimulus for its professorial body, rendering fixed payments very low, but leaving a wide scope for eventual emoluments. In France, on the contrary, the fixed payments constitute the main resource, while any eventual payments, in the shape of fees for examinations, are of little account. In Germany, again, the fixed payment varies at different universities, and for different professors at the same university. In the case of an important professor it sometimes undergoes increase in order to retain his services when he has received a call from another university where the payment is higher. This is a system of competition well calculated to maintain the ardor for work, and zeal for instruction. In France the professor is immobilized in the same university, and receives no assurance of competent remuneration for sustained labors; consequently, for him, the obtaining his chair constitutes his grand effort, and all serious work too often terminates with the successful *concours*. In Germany the future lies widely open to the professor, while in France it is most limited, the obstinacy with which the Paris faculty has always closed its doors to all provincial notabilities being well known.

The professors to whom chairs at Strasbourg have been offered have, before accepting these, repaired to that city in order to ascertain whether all the appliances for teaching were suitable; and some of these, it is said, have declined the appointments because they did not seem to them to be so. However, it is said that such will be amply provided, in the shape of laboratories, libraries, etc.; but some difficulty lies in the way with respect to clinical teaching. It seems that the Civil Hospital at Strasbourg has been founded, and is maintained, by private benevolence, its income of about 750,000 francs not being derived in any degree from the state or municipality. By a voluntary arrangement it has allowed this hospital, consisting of about 1,000 beds, to be employed by the faculty for clinical instruction; but, not regarding this arrangement as a contract, the committee in whose hands the management of the hospital rests believes that it has full power to withdraw this concession should it consider it as injurious to the patients confided to its care. It does not question the ability of the new professors to treat the patients, but looks to the great antipathy which the patients feel at being obliged to receive their services in place of those of medical officers who have long enjoyed their confidence and esteem. The question remains at present in suspense, but, in the event of the committee persisting in their refusal, the Germans will have to resort to the military hospital which now belongs to them, or to establish a new clinical hospital.

The chairs that have thus far been filled up are those of Anatomy, by Prof. Waldeyer; Physiological Chemistry, by Hoppe-Seyler, from Tübingen; Pathological Anatomy, by

Recklinghausen, from Wurzburg; Materia Medica, by Schmeidberg; Surgery, by Lücke, from Bern; Gynæcology, by Gusserow, from Zurich; Internal Pathology, by Leyden, from Königsberg; Psychiatry, by Kraft-Ebing; and Ophthalmology, by Laquear, from Lyons. The "rump" of the French medical faculty which remains at Strasbourg will, it is stated, complete the education of their former students, but has entered into no definitive relations with the new university. The bulk of this faculty, which has vacated its chairs, does not seem hitherto to have met with a very encouraging reception in France, beyond having received warm praise for the sacrifices which it has made.

Tea-Drunkards.—Dr. Arlidge, one of the pottery inspectors in Staffordshire, has put forth a very sensible protest against a very pernicious custom which rarely receives sufficient attention either from the medical profession or the public. He says that the women of the working-classes make tea a principal article of diet instead of an occasional beverage; they drink it several times a day, and the result is a lamentable amount of sickness. This is no doubt the case, and, as Dr. Arlidge remarks, a portion of the reforming zeal which keeps up such a fierce and bitter agitation against intoxicating drinks might advantageously be diverted to the repression of this very serious evil of tea-tippling among the poorer classes. Tea, in any thing beyond moderate quantities, is as distinctly a narcotic poison as is opium or alcohol. It is capable of ruining the digestion, of enfeebling and disordering the heart's action, and of generally shattering the nerves. And it must be remembered that not merely is it a question of narcotic excess, but the enormous quantity of hot water which tea-bibbers necessarily take is exceedingly prejudicial both to digestion and nutrition. In short, without pretending to place this kind of evil on a level, as to general effect, with those caused by alcoholic drinks, one may well insist that our teetotal reformers have overlooked, and even to no small extent encouraged, a form of animal indulgence which is as distinctly sensual, extravagant, and pernicious, as any beer-swilling or gin-drinking in the world.—*Lancet.*

Air in the Veins.—M. Trélat related, at a meeting of the Société de Chirurgie of Paris, an important case in which sudden death occurred in a patient from whom he was proceeding to remove a submaxillary tumor. The patient changed color, and the heart-sounds ceased suddenly. Artificial respiration and electrization of the phrenic nerve induced some respirations and a slight return of color after fifteen minutes, but ineffectually. At the necropsy, a small vein, opening into the external jugular, was found to have been partially divided; in the jugular was a long clot, segmented by air-blebs, and other bubbles of air were found in one of the mediastinal veins and the posterior cardiac vein, and a very notable quantity of air in the right cardiac cavities. The question raised was, whether the death was due to this cause or to the action of the chloroform. M. Perrin and M. Marc Sée attributed the death to chloroform; and M. Giraldès believed that the entrance of air in the veins acted with the chloroform in producing the fatal result. He stated that, in three cases of death from chloroform, he had found gases in the heart, in the vena cava, and even in the veins of the pelvis. Roux had made a similar observation. But M. Depaul pointed out that the air in this case occupied only the veins going to the heart, and the wounded vein. M. Perrin and M. Depaul were much in favor of artificial respiration by a tracheal catheter and bellows.—*British Medical Journal.*

An Heroic Druggist.—The Paris correspondent of the *Lancet* relates the following incident: The anniversary of the entry of the Versailles troops into Paris painfully reminded me of the dreadful scenes I had witnessed and the unheard-of atrocities committed during the furious struggle between the insurgents and the regular army. Among the melancholy events that then took place I may mention that related of a *pharmacien* in the Rue de Richelieu by the name of Koch, who was brutally murdered by the Communist soldiers simply because he refused to help them in raising a barricade in his neighborhood. Not only did he refuse his aid, but he attempted to lecture them on their conduct, whereupon two of the men attacked him in his own shop. The pharmacien, however, true to his drugs, kept them at bay with a bottle of sulphuric acid in his hand, which he threatened to bespatter them with if they dared to touch him. 'The men, finding a dangerous weapon before them, beat their retreat, but soon returned with a reënforcement. The poor pharmacien, considering it would be useless to resist, was carried out of his shop, and, after a sham "drum-head" court-martial, was ruthlessly shot in the presence of his wife and children, who implored the ruffians for mercy.

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Original Communications.

ART. I.—*A Case of Rupture of the Urinary Bladder, with Remarks upon the Treatment of this Accident.*[1] By ERSKINE MASON, M. D., Adjunct Professor of Surgery, etc., in the University of New York.

MICHAEL BURKE, aged twenty-six, Ireland, single, laborer, was admitted into the Roosevelt Hospital, at midnight, on December 26, 1871, complaining of inability to urinate, although having an urgent desire to do so. He stated that at noon, on December 25th, he was seized with an attack of dizziness, and fell down a long flight of stairs. What portion of his body he struck in his fall he was unable to state. He complained of pains about the hips, some tenderness over the hypogastrium, and frequent desire to urinate. Mind clear, temperature 98⅗, pulse 70, and small. Upon inspection, both eyes were found ecchymosed, and the lids of the left eye much swollen; there was also a small cut upon the left upper lid just beneath the eyebrow. Beyond a few abrasions on the arms and legs, no other wound was detected.

A No. 10 (English scale) silver catheter was passed, immediately upon his admission into the hospital, by the house-surgeon, Dr. Schuyler. The instrument entered the bladder with ease, but only a few drops of water passed, and this was streaked with blood. Patient being in great pain, ♏x of Magendie's solution of morphia was given hypodermically, which caused him to sleep during the night. The following morning (December 27th) the catheter was again readily introduced, and drew off ℥iv of urine, with some blood. Tenderness over the abdomen seemed to be increasing. In the afternoon an injection of an ℥j of oleum ricini was given, but it caused no movement from the bowels. I saw him for the first time that afternoon about 4 o'clock. His condition was then, perhaps, more comfortable than it was a few hours previous. He was at this time able to walk to the water-closet at the end of the ward, and void a small quantity of urine. His expression of countenance was anxious and very pale—tongue somewhat furred; very thirsty, and with a pulse of only 68, small, and temperature 98⅗. I at this time introduced a catheter readily into the bladder, and drew off some clear urine; then introducing my finger into the rectum, satisfied myself that no injury, such as laceration, had occurred to the urethra. During the examination *per rectum* I thought I detected a swelling posterior and a little to the left of the prostate, but on this point I did not feel quite satisfied. The patient now was complaining of great pain in the lumbar region, and I was unable to decide positively if rupture of the bladder had occurred, or whether he was suffering from some injury of the kidneys, and the pain over the suprapubic region was not due to a blow he had there received. Had I been able there and then to make my strong suspicions of rupture of the bladder a little more positive, I should have at once resorted to the operation which I afterward performed.

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[1] Read before the New York Journal Association, April 26, 1872.

I therefore gave directions for the patient to remain in bed, never to try and urinate himself, to have the catheter passed every hour if necessary, to be kept quietly under morphia, have a camphor stupe applied over the abdomen, and, if any symptoms changed for the worse, that I should be sent for at once.

At 10 P. M., when the house-surgeon drew off his water, there were no changes in his symptoms; ♏x of Magendie's solution was at this time given hypodermically.

December 28th, 3.15 A. M.—He was very restless and thirsty; pulse 112, temperature 102°. A small amount of urine was then drawn, and for the first time the doctor found some difficulty in passing the catheter; abdomen now was hard and extremely painful; ♏ x of solution morphia administered as before, and Vichy water given as a drink.

5.30 A. M.—Patient sleeping.

6.30 A. M.—Water again drawn, though only a small quantity; pulse 120, small; temperature 101°. Same difficulty as before experienced upon introduction of the catheter.

9 A. M.—A little bloody urine was drawn off; pulse 112; temperature 99⅘°. Has not as much pain in the abdomen, though considerable tympanitis is present. Dr. Schuyler now called at my office, and reported the condition of the patient. I at once went up to the hospital, reaching there at 10 A. M. I found the patient lying in bed, with his limbs drawn up, great tympanitis, hiccough, and vomiting, excessive tenderness over the whole abdomen; with a small, wiry pulse of 120, coated tongue, great restlessness, and cool extremities.

There could be now no doubt that the case was one of rupture of the bladder, and complicated by general peritonitis; and that, if there was any thing to be done by operative procedure, there was no time to be lost. I therefore determined to put in practice what I had some time previously made up my mind was the proper course to pursue in just this class of injuries, viz., to lay open the bladder through the perinæum, as in the lateral operation for stone, and thus give a free exit to the urine.

Placing the patient under the influence of ether, I again examined him thoroughly *per rectum*, and convinced myself that there was no laceration, either of the urethra or neck of the bladder around the prostate. Posterior to the prostate, and to the left, a decided tumor was felt, which to my finger communicated the sense of fluctuation; there was no thickening or induration of the tissues anterior to the neck of the bladder. A large-sized staff was now passed into the bladder with the greatest facility, and I laid open the bladder, the same as in the lateral operation for stone. As soon as the bladder was opened, a large quantity of bloody urine escaped. Passing my finger into the bladder, so as to enlarge the opening, I felt confident I detected a rent in the posterior wall of the viscus, but I did not examine this opening thoroughly, as I feared I might do injury did I pursue investigations further in that direction, being convinced that we are very liable to do irreparable injury by too much fingering of parts after delicate operations.

Two points here noticed are worthy of observation:

1. That upon introducing the finger into the bladder the temperature inside the organ was perceptibly lower than the surface of the body, or the cut edges of the wound, and this fact was observed also by Dr. Sizer, our senior assistant.

2. That the tumor, felt through the rectum, behind and to the left of the prostate, disappeared after the bladder was opened. There being some venous bleeding from the wound which ice did not satisfactorily control, a large flexible catheter was passed through the wound into the bladder, and the wound tamponed with lint. The patient was then removed to his bed, a warm camphor-stupe applied over the abdomen, and a dose of opium ordered.

My diagnosis was now that the rupture had taken place through the posterior wall of the bladder, that the rent had extended through the peritoneal covering, and that the urine had extravasated into the pelvic cavity, but rested chiefly in the posterior *cul-de-sac*.

What the obstruction was which the house-surgeon met with in his later attempts to pass the catheter, I am at a loss to discern, unless it were due to an extremely contracted condition of the bladder being present on these occasions. Urine flowed freely through the catheter, and at 3 P. M. he complained but little of pain, indeed was quite comfortable; pulse 108, weak; temperature 98⅗°.

7 P. M.—Has a little pain over the bladder; pulse 116; temperature 101⅗°; ate some farina-and-milk for his supper.

10 P. M.—Much easier; abdomen less tense and less tender; sleeping.

29th, 9.30 A. M.—Passed a comfortable night, feels "easy," has had a slight movement from the bowels; pulse 100; temperature 101⅘°.

3 P. M.—Had another movement from the bowels; abdomen more tympanitic and painful than at last note. Spongio piline applied over the abdomen, and ʒj of U. S. solution of morphia was directed to be given p. r. n.

7 P. M.—Pulse 100; temperature 102°. Abdomen distended, though not very painful; respiration ·24. Diet consists of milk, which he takes readily; he complains somewhat of tenesmus; urine passes freely through the catheter; a brown discoloration is now observed over the inguinal, hypogastric, and perineal regions, and down the thighs; a pill of half a grain of opium ordered every three hours.

December 30*th*, 9 A. M.—Pulse 92; temperature 100⅗°; respiration 20; but little pain; abdomen less tense, tongue slightly furred; had natural movement of bowels this morning; catheter removed.

12 M.—Urine flows freely, and is clear; wound looks well; opium ordered to be taken so as to endeavor to keep the respiration down to 20. Pulse 88; temperature 100⅗°; respiration 23.

3 P. M.—Pulse 92; temperature 101⅘°; respiration 30. Those portions of the body previously spoken of, as presenting a dark-brown appearance, present a hard and indurated feeling. Abdomen a little more tense than yesterday. Patient is bathed in profuse perspiration, which has a strong urinous odor; half grain of opium ordered every two hours.

December 31*st*, 9 A. M.—Pulse 84; temperature 99½°; respiration 24. Urine passes freely by the external wound. Feels better and stronger. Abdomen is less tense, and discoloration is fading.

3 P. M.—Pulse 80; temperature 100°; respiration 25. Very little pain from pressure over the abdomen, takes nourishment well; still has considerable perspiration, which is of the same strong urinous odor.

January 1*st*, 9 A. M.—Pulse 80; temperature 99¾°; respiration 22. Feels much better, and so continued through the day, with less perspiration than the day previous, however.

Passed a large quantity of clear urine through the wound, which had a strong ammoniacal odor.

January 3d, 9 P. M.—Pulse 84; temperature 102°; respiration 24. Says he feels quite well, appetite good, wound looks well, urine high-colored, and of a strong ammoniacal odor; still has perspiration of the same character as before. All swelling of the abdomen rapidly disappearing. Opium discontinued.

January 8th.—Since last date patient steadily improving. Pulse this morning 90; temperature 98⅖°; respiration 21. Begins to pass some water per urethra; improved steadily, so that on January 11th he was dressed, and sat up for a little while.

January 25th.—Passes almost all his water by urethra, but is troubled with great irritability of the bladder, so that he is compelled to pass water very frequently. Ten drops of tinct. of ergot, with ten drops of tinct. ferri mur., ordered three times a day, and under this treatment his bladder-difficulty rapidly abated.

February 2d.—Feeling quite well, he desired to be discharged, so as to go to work.

February 3d.—I passed a No. 12 sound into the bladder, which met with no obstruction in its passage, nor caused the slightest pain to the patient; he was therefore discharged from the hospital, cured, on the thirty-ninth day after the accident, and the thirty-seventh day after the operation.

I give the history of this case in full, on account of its being comparatively a rare though most serious form of injury, and because it illustrates so well the efficacy of the mode of treatment which I desire to advocate.

The bladder, its diseases and injuries, has always presented peculiar attractions to the surgeon.

With reference to its diseases, much has been written, and great have been the advances made by the profession during the past few years in the treatment of the varied disorders which this organ is liable to. As to its injuries, and especially rupture or laceration of its walls, the result of external violence, we have not made the same progress in respect to treatment as has been effected in regard to its diseases; and, though no longer believing, as did the ancients, that all wounds of the bladder are necessarily fatal, yet, from the days of Hippocrates down, this class of injuries has to be classed among the most fatal with which we have to deal—and particularly rupture the result of external violence. To convince ourselves of the truth of this assertion, we have but to glance at the statistics of this injury, as given by Mr. Birkett in Holmes's "Surgery," and by Prof. Stephen Smith, of this city, in a paper "On Rupture of the Urinary Bladder," which appeared in the *New York Journal of Medicine* for 1851 (vol. vi., p. 374). In the article by Mr. Birkett we find that the "records of fifty examples of this injury show that, with the exception of three, all the cases terminated fatally." Of these three examples of recovery, the symptoms of only one were those of extravasation of urine into the peritoneal cavity; one the extravasation was into the connective tissue of the pelvis, complicated with fracture of the pelvic bones; while the third case was of the same nature, but without the complication of fracture. In Dr. Smith's paper—which, without doubt, is the best article that has been written upon this subject—we find a table of seventy-eight cases, and among this large collection but five recoveries are reported. In three of these, the extravasation was into the cellular tissue, but one into the peritoneal cavity, and one reported as partial; and in but three of these cases was the rupture due to external violence. The cause of rupture in the cases of recovery cited in Holmes's "Surgery" is not stated. The cause of injury in the seventy-eight cases just referred to was, direct violence in forty-eight; concussion, fifteen; internal causes, nine, four of which were from stricture; four, parturition; one, retroversio uteri; and in six the cause is not mentioned.

As to the location of the rent, it has been found in far the greater number of cases to have occurred in the posterior wall of the organ, and, as a consequence, the urine has passed into the peritoneal cavity. Next in order of frequency we find it at the anterior wall, and, when here situated, the extravasation is into the areolar tissue, and rarely has it been observed at the *bas-fond*. But one opening, as a rule, occurs, yet cases are referred to where more than one laceration has taken place.

Neither sex nor age proves a barrier to this accident, it having been observed in the fœtus as well as in the man of advanced years. The reason it is seen more frequently in the male than in the female is sufficiently obvious; and it has been observed more frequently between the ages of thirty and forty.

Of the cases that resulted fatally, death has most frequently followed within five days; though, in one very remarkable case, reported by Dr. E. R. Peaslee (*American Journal of Medical Sciences*, vol. xix., p. 383, 1850), the patient, a man thirty years of age, survived forty-two days, the laceration being situated at the neck of the bladder, and complicated with wound of the perinæum and fracture of the pelvic bones. Extravasation was evidently into the areolar tissue, and large abscesses were found after death in both iliac regions.

With reference to the symptoms which denote this accident, they are too well depicted in our general works on surgery to require much notice in a paper of this kind. Many of our works lay it down as a rule that there is inability to void urine on the part of the patient. That this is not always the case, at least after a few hours have elapsed from the time of injury, the case I report, as well as some others, sufficiently proves. Inability to walk about is also often observed from the first—in my case this symptom was absent. Some bruise or laceration is often noticed over the hypogastric region—none was here observed. That the patient struck upon the abdomen was sufficiently obvious, from all the wounds and abrasions upon the face and limbs being upon the anterior aspect of the body. Two symptoms which presented themselves in the case which is related, and which were watched with great interest, were: first, the dark-brown discoloration of the hypogastric, inguinal, and perineal regions, which appeared on the fifth day after the accident, and which passed off without proceeding to suppuration; secondly, the occurrence of profuse perspiration over the whole surface of the body, which took place on the fifth day, and which continued for some three days, its odor being so strongly urinous that it was perceived at once upon entering the ward, and, to render the patient less objectionable to the surrounding patients, carbolic acid had to be used about his bed.

With reference to the first of these symptoms, I have seen no mention of it in the cases I have read. As to the other symptom, the only writer I find who mentions it is Baron Larrey in his "Surgical Memoirs of the Campaigns in Russia, Germany, and France" (translated from the French by John C. Mercer. Carey & Lee, 1832, p. 180), while speaking of wounds of the bladder. No doubt through this means much of the urine which had been extravasated into the peritoneal cavity was eliminated.

Respecting the prognosis in cases of injury of the bladder, it never can be otherwise than very grave, yet its gravity will be modified, both from the character of the wound, and the parts invaded by the extravasated urine. Gunshot-wounds, for example, are far from being so uniformly fatal as formerly supposed, as the records of both military and civil surgery amply testify; while the wounds which we denote as incised and punctured far more frequently result fatally, and hence the remark of Hippocrates: "*Cui persecta vesica lethale.*"

So with reference to that form of accident of which we specially speak, we find our prognosis is materially influenced by the locality of the rupture, and course the extravasated urine has taken, for it will be remembered that, of the fifty examples recorded in Holmes's "Surgery," of the three recoveries that followed, in only one did extravasation of urine take place into the peritoneal cavity, and, in the five cases of recovery among Dr. Smith's cases, but one gives the history of urine extravasated into the cavity of the peritonæum, one of partial extravasation, or partial rent of the peritonæum; in the remaining cases, it was only the connective tissue which was involved.

Of course the locality of the rupture will have great tendency in directing the course of the urine. If it is the anterior wall that ruptures, then it will be the connective tissue that chiefly suffers; whereas, if the posterior wall is the portion that gives way, the peritonæum will be the portion involved in far the greater number of instances, and fatal peritonitis will rapidly ensue.

An opposite view of the nature of these cases, however, is given by Blundell ("Observations on some of the more Important Diseases of Women," by James Blundell, M. D., Dunglison's American Medical Library, Philadelphia, 1840, p. 54), who says: "If the urine is extravasated in front, I fear that there is little to be done: inflammation, sloughing, and death, are successively the fate of the unhappy patient." He recommends that an opening be made above the pubis, the extravasated urine and blood removed, and the thorough washing out of the abdominal cavity by means of the free injection of distilled water at the temperature of 98° Fahr., and this to be continued till the water which flows away bears no trace of urine; for, he says, if the urine remains in the peritoneal sac, fatal inflammation will follow. The ruptured part of the bladder was then to be drawn up to the abdominal opening; the laceration then closed by a ligature, those portions of tissue beyond the ligature being carefully cut away; the bladder is then to be drawn up by means of the ligature to the abdominal opening internally; one end of the ligature is to be cut away, the other end to lie out of the wound to separate and be withdrawn afterward, as when an artery is tied. In pursuing such a course he believes that in some few cases life might be preserved.

With a view to test this theory, he experimented upon four rabbits. About two ounces of human urine being thrown into their abdominal cavities, it was allowed to remain there for an hour; then it was withdrawn, and the cavity well washed with tepid cistern-water. Three of these animals died with general peritonitis, while the fourth lived. In another class of experiments he tied up the fundus of the bladder in the rabbit, afterward cutting the fundus away; the ligature, he found, came away in a few days, leaving the bladder closed, though some of the rabbits perished some months afterward in consequence of chronic disease, not apparently the necessary, but the accidental effect of the experiment. He finally believes that this may succeed in the human subject, and, as an additional inducement for its being tried, cites Mr. Travers as having tied up with success a small aperture in the stomach.

For the treatment of this grave accident, what are the resources which we may call to our aid?

If we consult the ordinary text-books on surgery, we might infer at once that, no matter what we do, death will most certainly ensue within a very few days at the farthest. Thus Gross, in his work on the urinary organs (p. 146), says: "It is obvious, from what has been already stated, that no measures, however well directed, will, in general, be of any avail in saving life."

The indications to be met are very apparent, viz., to prevent extravasation of urine, and subdue inflammation. In far the greatest number of cases, as their records will show, extravasation has happened upon the very receipt of injury, so that at best we can only prevent its secondary recurrence. The stereotyped mode of accomplishing this, as handed down from one book to another, and from teacher to pupil, is to prevent the patient assuming the erect position, or making any effort to void his urine. A catheter, and at the present day (very wisely, we think) one of flexible character, with an eye at the point as well as at the sides, is to be introduced, so that whatever urine may remain in the bladder may be removed, and this instrument is to be either frequently introduced, or else allowed to remain just within the neck of the bladder for a variable period of time, so urine may escape as fast as it enters the bladder. This course of treatment, we are told by Mr. Birkett (Holmes's "Surgery," vol. ii., p. 484), should be persisted in for *not less* than fourteen days after the receipt of the injury. Should any sign of extravasation appear externally, free incisions are to be made, to allow the escape of fluid, and facilitate the casting off of sloughs; while inflammatory action is to be controlled either by calomel, blisters, local bloodletting, or opium, the latter being now chiefly relied upon.

This will be found to be the sum and substance of treatment not only laid down in text-books, but the mode of practice usually pursued in our hospitals. The frequent introduction of the catheter, or the confining the instrument within the bladder for any length of time, is attended often with serious inconvenience, which is too well known, not only to the patient but also to the surgeon, to require more than a reference at the present time.

The desire on the part of surgeons to render this accident less of an opprobrium to their art has led some from time to time not only to suggest but put in practice other forms of treatment—and with respect to some of these, it strikes us they have never been fairly tried—abandoned, it may be, on account of temerity on the part of practitioners who have preferred to allow a patient to die, in an orthodox manner, rather than walk over ground that was too new, or not sufficiently broken for their *cautious feet.* Perhaps other methods of treatment may have been forgotten, or they may not have been familiar to some authors or surgeons. The latter we must suppose has frequently been the case, as we so seldom see them referred to in works on surgery.

The various methods of operative interference which we now allude to, which have either been proposed or put in practice, with the hope of better meeting the necessities of the case, and thus rendering this accident less fatal than it has proved under the accepted and old-established course of practice, may be grouped together under the following heads:

1. Opening the bladder above the pubes, as in the high operation for stone, or else simply tapping the organ in this locality.
2. Tapping the cavity of the pelvis either above the pubis, or the pelvic *cul-de-sac* through the rectum.
3. Perineal section, and then dilating the membranous portion of the urethra and neck of the bladder, as in the median operation for stone.
4. Opening the bladder either by the lateral or bilateral method, as in lithotomy.

The first method, viz., opening the bladder above the pubis, at first sight, especially where the anterior portion is supposed to be the part ruptured, and the urine has extravasated into the areolar tissue, might strike us as being very timely, as through this means both extravasated blood and urine might readily be removed. But when we consider that extravasation as a rule takes place rapidly, and at the moment of laceration, and, as a natural consequence, collapse—more or less extensive—of the viscus will immediately ensue, the value of this operation, as a means of preventing secondary trouble, to our mind, is not very great. Indeed, it seems that about all that we might hope to accomplish, in the majority of cases, could be obtained through the simpler means of free and deep incisions made in this region.

The danger alone of wounding the peritonæum, if not already injured in our attempts to open a collapsed or contracted bladder in this locality, and thus adding a serious complication to an injury already sufficiently grave, would be enough to make us hesitate in selecting this course of procedure, especially when another seems to hold out more favorable results.

As far as I am aware, this operation has been put in practice but once, and this at the late New York Hospital, in a case of supposed rupture of the anterior part of the bladder. Here the peritonæum was wounded in the operation; some hours afterward perineal section was performed, and the organ was then discovered not to have been lacerated, but, as the autopsy subsequently showed, rupture of the urethra close to the neck of the bladder had taken place.

The risk that we may be in error as to our diagnosis in some cases, as the above case shows, must also militate against this operation.

With respect to paracentesis either above the pubis or of the pelvic *cul-de-sac* through the rectum, which is the second method we have spoken of. Paracentesis above the pubis, so far as I can learn, has been resorted to but twice: First, according to Dr. Harrison ("Cases of Rupture of the Urinary Bladder, with Remarks by Robert Harrison, M. D., etc.," *Dublin Journal of Medical Science*, July 1, 1836), by Bonetus, blood only flowing through the trocar, and the patient dying forty-two hours from the time of the accident. The second case was by Dr. Cusack ("Dublin Hospital Reports," vol. ii., 1818, p. 312), in February, 1814. The operation .was done on the third day after the accident, fluctuation being distinguished in the abdominal cavity; a large quantity of urine of a natural color and perfectly transparent was drawn off, and the patient expressed himself relieved. In this case the gum-elastic catheter was also secured in the urethra. On the sixth day the patient was distressed by frequent involuntary seminal emissions, and the urine had ceased either to flow from the wound above the pubis, or through the catheter; and he died on the morning of the eighth day. The rupture in both of these cases was in the posterior portion of the bladder. This mode of operating certainly would not lead us to hope that much could be obtained from it, as it does nothing toward preventing further extravasation, and both cases in which it was tried, we have seen, proved fatal.

Boyer (Maladies Chirurgicales, tome ix., p. 61), while remarking upon a case of lacerated bladder, says that no relief can be afforded in this class of injuries—the paracentesis of the abdomen, and retaining a catheter in the bladder, are the only surgical efforts that can be attempted; but, before paracentesis can be performed, the fatal peritonitis has sealed the doom of the patient.

With this opinion Dr. Harrison says he does not fully concur, his own being that in paracentesis we are to find the great remedial agent; but, as the effused urine is chiefly found in the pelvic *cul-de-sac*, we are to tap this locality; and he has therefore proposed that in these injuries this *cul-de-sac* be tapped through the rectum. "Should the parts," he says, "be in that state in which dissection has shown them in some cases to have been, we may suppose that the pelvic *cul-de-sac* is distended with fluid, coated with lymph, and well protruded toward the rectum, or between this and the bladder; that the latter viscus is empty and rather small, and that adhesions have nearly closed the pelvis above, and separated it from the abdomen; if a small opening be now made through the rectum into this *cul-de-sac*, the irritating fluid may be discharged without injury to any important part; indeed, the opening into this new and circumscribed cavity cannot even open into or affect the general peritonæum, provided the superior pelvic and vesical adhesions have been perfect; in fact, a new cavity has been formed, coated internally like an abscess, and containing a foreign and an irritant fluid, and the operation now suggested is merely opening this in the most depending situation."

In addition to this, he recommends retaining the catheter in the bladder, and the free administration of opium. This operation Mr. Harrison believed to be in accord with the sound principles of general pathology; he had no experience in its favor, but was led to this view from the appearance which the autopsy of several cases had presented.

This mode of treatment was proposed to the profession in 1836, but we are yet to learn that it has ever been put in practice.

At first sight, it certainly may appear very plausible, and in theory, as regards the necessity of giving vent to extravasated urine, it undoubtedly is correct. The objections to this operation strike us as follows: It, in the first place, is not suitable to all classes of cases; for we must bear in mind that, though in the great majority of cases of ruptured bladder the rent is in the posterior portion and through the peritonæum, yet there is a proportion of cases where the laceration is superior or anterior, and, the peritonæum not being wounded, the urine does not collect in the pelvic *cul-de-sac*, and we are not always able to say to which variety a case may belong. Again, in the performance of this operation, we fear that the bladder might be opened rather than the *cul-de-sac*, there being a number of cases in which the autopsy has disclosed this organ, though torn, to contain considerable urine; and this fact was evident in our case as well as others we have seen reported, from the ability of the patient to pass water himself.

Another danger lies in the risk we may run of wounding a coil of small intestine or the rectum itself. There would be no danger in doing this, it is true, if this sac were so completely shut in and closed above as Dr. Harrison's observations led him to think was often the case.

But do we often find such a condition of things as Dr. Harrison describes? In the paper of Dr. Harrison we find but one case given, and that occurred in his own practice, where the pelvic *cul-de-sac* was entirely shut off by adhesions from the abdominal cavity, and one case which he cites as occurring to Dupuytren where such condition almost existed. One of these cases died on the seventh day after the receipt of injury, while the other lingered until the eighth day. This was also the case in a patient under the care of Dr. Cusack ("Dublin Hospital Reports," vol. ii., p. 316), who lived eight days.

These are the only three cases out of seventy-eight, as collected by Dr. Smith, where the *post-mortem* appearances revealed such a condition of things, and it is very evident that Nature could only so protect herself after an interval of some days; whereas, were we to wait for these adhesions to form, in by far the greater number of cases death would relieve the patient before the surgeon.

Though the proposal of this operation was a great step in the right direction, it does not meet all the requirements of the case. It does not obviate the necessity of constantly retaining the catheter in the bladder, and this to us is objectionable. If the catheter is not constantly watched, and this is not always possible, we know how very liable its eyes are to become occluded, either from blood-clots or mucus, and then, if the rent in the bladder is low down, what have we to prevent urine again passing into the peritoneal cavity? Even under the most favorable circumstances, the catheter will not fully protect against this accident.

In respect to the *third* mode of treating these cases, namely, by perineal section, and then dilating the membranous portion of the urethra and neck of the bladder with the finger, as in the median operation for stone, we know not by whom it was first suggested, though we are aware that it has been followed in at least one case in this city, and probably in several.

This case was in St. Luke's Hospital, and was operated on by Dr. Robert F. Weir. The accident occurred in a middle-aged man, by his being caught between a ferry-boat and the bridge. Upon introducing the finger into the bladder, the doctor discovered that the pubic bones were fractured, and projected through the anterior wall of the bladder. This case resulted fatally.

Though this operation is a decided advance in treatment over the one we previously considered, yet its disadvantages seem to us to consist in the fact (which is claimed as one of its advantages by the advocates for the median operation in lithotomy), that after this operation the patient is not troubled by the urine dribbling away from him, and, if not at once, he very speedily possesses control over his bladder.

That this is the case, no matter how thoroughly the neck of the bladder is dilated, unless it be lacerated in the removal of the calculus, all are aware, who have had experience in the median operation for stone. This, then, being the case, wherein by this operation have we taken any steps to prevent the urine again soon finding its way through the rupture in the bladder? Our only plan to prevent such an occurrence as a consequence of this operation, it appears to me, would be frequent dilatation of the neck of the bladder, and this certainly would be a most serious objection.

We now come to the *fourth* and last method which has been practised in these critical cases, and the one which we desire earnestly to advocate, viz., the opening of the bladder freely by means of the lateral or bilateral operation as for stone.

To American surgery belongs the honor of having given to the profession this mode of treatment; and to Dr. William J. Walker, of Boston, belongs the credit of having first put in practice, and, I believe also that of originating, this plan of treatment.

Dr. Walker's case was that of a man, aged twenty-three, of sound constitution, and of temperate habits, a railroad-conductor by occupation, who was caught, during an accident, between the engine and a car. When the doctor saw him, which was twenty-four hours after the accident, he presented the symptoms of a man passing into collapse. A tumor was observed extending from the whole line of Poupart's ligament nearly to the umbilicus, almost as large as the open hand, and elevated above the surface an inch and a half; the case was also complicated by fracture of the pelvis at the symphysis pubis. A catheter was introduced, and several ounces of urine withdrawn, whereupon the tumor referred to disappeared, and the bladder was then opened as in the lateral operation for stone, and immediate relief followed. The fracture was found to have united twenty-five days after the accident, and in fifty-five days from the time of injury the patient resumed his occupation. This case was regarded (and we think very justly) by Dr. Walker as one of laceration anteriorly and external to the peritonæum (medical communication to the Massachusetts Medical Society, Art. IV., Case VI. of vol. vii., 1845).

We have had our attention turned to this subject on several occasions, both from cases observed while a student, and while house-surgeon at Bellevue Hospital in 1860. It was not, however, until 1863, when Dr. Smith gave me a copy of his paper on "Rupture of the Bladder," that I became acquainted with this plan of treatment, which struck me as the most plausible that had been suggested, and from that time I determined to resort to it should a case ever come under my care, and, in my instructions to students, I have recommended it as one worthy to be followed.

We should feel that we were doing great injustice to Dr. Smith were we to omit stating that he, in the paper so often alluded to, while speaking of this operation, says that it "seems the most rational yet pursued." If we but consider for a moment, I think we shall be convinced that it is the most practical and common-sense method in dealing with these cases that has yet been proposed or practised, and one which, more fully than any other, meets the most urgent requirements of the case, viz., the giving vent to extravasated urine, and preventing recurrence of the same.

That it is of equal service, whether the rupture has taken place either at the anterior or posterior portion of the bladder, both Dr. Walker's case and my own fully testify. Again, in both cases in which it was tried it was successful, and this is more than can be said of any other treatment, especially when so complicated as each of these was—one from fracture of the pelvis, the other from general peritonitis, with extravasation of urine into the pelvic cavity.

These are the only two cases of which I can find any record of this operation having been done; and mine is the only one in which laceration was evidently in the posterior portion of the organ, with extravasation into the pelvic cavity, which recovered through means of operative interference. Should I be in error in this respect, I trust I shall be corrected.

To insure a greater chance of success from this operation, it appears needless to urge an early resort to it. Still, after some time has elapsed, we find it may prove successful in saving life, twenty-four hours having elapsed from the time of injury in the first case, and sixty-two and a half hours in my case, before the operation was done.

In a previous portion of this paper we stated that the records of gunshot-wounds of the bladder prove that they were far from being so uniformly fatal as generally supposed; whereas, in the cases of incised wounds, the result is almost uniformly disastrous.

The cause for this, I believe, is to be found in the fact that, in the former class of cases, the passage of the ball leaves a free opening through both the bladder and walls of the pelvis, whereby the urine is allowed a ready exit; while with the latter variety no such egress is obtained.

We find also cases reported where recovery has taken place, when abscesses have pointed in the perineal region, and having been opened, either by the surgeon or through natural processes, or where the abscess has burst into the rectum, and urine has flowed out through the sinuses thus formed. All this has occurred even where the sloughing was most extensive, and teaches us a lesson, I think, in the treatment of such cases.

When the rupture has evidently been through the anterior part of the organ, and symptoms of extravsation into the areolar tissue are present, in addition to the operation of Dr. Walker, we should resort to free and deep incisions through the œdematous parts.

When this accident happens in the female, the indications are equally obvious; and here we believe, if the bladder be freely opened through the vagina, our prospects of success would be greatly enhanced.

In conclusion, there are two more points of interest to which I would like to draw attention: First, in reference to the power of locomotion remaining for a while after the occurrence of the accident; and, second, the ability of the patient to void urine of his own accord. Both these functions, as a rule, are abolished at the moment of injury. In the case I report, it will be observed that the patient walked to the hospital, and was able to move about the ward on the day after his admission, though in a stooping position, and on this day he could at times pass water of his own accord, though in small quantities.

Looking over Dr. Smith's collection of seventy-eight cases, we find that the power of locomotion was present only in seven cases, and in but three to any considerable degree. The most remarkable case in this respect we find mentioned by Dr. Harrison (*Dublin Medical Journal*, vol. ix., p. 354, 1836), of a man who, though so weak as hardly to be able to walk immediately after the accident, soon rallied and went home without assistance. After spending a restless night, unable to pass water, he walked a distance of three miles to a surgeon, who drew off some water for him, when he again walked home. This accident happened on a Saturday night, and, though feeling very bad, he went to work on Monday, but, about twelve o'clock, feeling very ill, with pain in the belly, he took a glass of whiskey and again walked to see the surgeon, but, on his way back, was obliged to ride part of the distance. Death occurred in this case on the eighth day. The autopsy revealed an oblique fissure through the posterior surface of the bladder, the edges of which were thickened and slightly adherent, so that the doctor was unable to press any of the pelvic fluid into the bladder. In the pelvic cavity was found nearly a quart of urine; this was shut in from the abdominal cavity by adhesions.

With reference to the ability on the part of the patient to void urine, we find that in only three cases did it exist immediately after the accident, and in only a comparatively small proportion of cases did it ever recur, except, of course, in those who recovered.

In closing, I take this occasion publicly to acknowledge my indebtedness to Dr. Schuyler for the watchful care he bestowed upon my patient, to Dr. Sizer, our senior assistant, for the very accurate notes which he furnished me of the case, and to Dr. Samuel S. Purple for kindly placing at my disposal valuable works of reference contained in his library.

ART. II.—*On the Treatment of Syphilis.*[1] By ALFRED FOURNIER, Physician to the Lourcine Hospital, Paris. Translated by R. W. TAYLOR, M. D., Surgeon to the New York Dispensary, Department of Venereal and Skin Diseases.

PART I.—ON THE NECESSITY OF TREATING SYPHILIS, AND ON THE ACTION OF MERCURY.

THE peculiar opinions which have recently been put forth as to the treatment of syphilis have neither overturned nor even unsettled the results of the observation and labor of four centuries. The trifling opposition which has been raised against mercury, and in a more general manner against the methods of treating syphilis, has succeeded only in rallying a small number of converts, and has invoked only very feeble arguments to its aid, so that it will scarcely leave any traces of itself in the history of medicine. Mercury, to speak of it alone, has already encountered much more serious opposition, and much more powerful enemies. In the sixteenth century guaiac, was vaunted against it; but it failed finally to replace it, although for a time, in consequence of the powerful support of medical and non-medical writers, it did outrank it. In the present century the physiological school, which could not find enough invectives to apply to it, and which proclaimed its extinction, did succeed in banishing it from use for a few years, but it has survived, and will survive, in consequence of the experience and observation of its undeniable virtues; for, whatever may be said against it, it is a great and beneficial remedy, which cannot be injured by the prejudiced calumnies of the one, nor by the unjust but conscientious accusations of the others.

Previous to a sketch of the treatment applicable to syphilis, I will consider the question as to whether there is any necessity to treat syphilis, or whether it is well to abandon the disease to its natural course; and, before explaining the manner of using mercury, I will inquire whether there are any benefits to be derived from it, and whether its introduction into the economy is attended with danger.

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[1] This article has recently appeared as a clinical lecture in the pages of the *Gazette Hebdomadaire de Médecine et de Chirurgie.* In its translation it has been found necessary, in consequence of its colloquial style in some places, to slightly alter the diction, and in others to leave out unimportant repetitions; but every care has been taken to present the full meaning of the author in a clear manner.

From what has been recently said and written, it would appear that syphilis was one of the most mild and benign of diseases. According to the views of certain contemporary physicians, a person would suppose that the disease cured itself in consequence of an inherent tendency thereto, and that it was only necessary to aid Nature in its spontaneous elimination of the virus by hygienic influences, and that hygiene alone, aided or not by tonics, as the case might require, would be all-sufficient, and that any special treatment, or any specific remedy against the diathesis, was both visionary and superfluous.

This is not at all exaggerated, as will be found by reading the recent discussions upon the subject, in which statements like the following may be found, some of which, by-the-way, are contradictory: "Syphilis cures itself spontaneously—whatever may be done, it has an inevitable evolution and duration; the remedies usually prescribed for it tend neither to modify nor shorten its manifestations; it is even injurious to treat it, for that only disorders it, and retards its natural evolution, and hinders it from running itself out spontaneously; when left to its natural course, syphilis is mild, and it only occasionally results in serious lesions when an injurious treatment has been adopted for it; the tertiary manifestations need not be feared, for, on the one hand, they are very rare, if the physician has the good sense to allow the diathesis to run itself out spontaneously and naturally by its mild secondary manifestations, and because, on the other hand, we possess a certain specific for them; mercury, the so-called specific *par excellence* for syphilis, is not only inert, as it does not cure either the disease or its lesions, but it is even dangerous, because it acts as a poison upon a system already poisoned; finally, the expectant treatment pure and simple, aided by suitable hygienic measures, and according to the case by tonics, is the most rational and surest method that we can adopt for primary and secondary syphilis, indeed, even (as one of my colleagues recently said to me) for tertiary syphilis. All these arguments resolve themselves practically into this: Is it or is it not necessary to treat a syphilitic patient? Is it or is it not beneficial that he should be treated?

In order to answer a proposition thus stated, let us consider what risks such a patient runs, by stating his condition clearly. To what dangers, in fact, is he exposed? Let us set forth his pathological balance-sheet, if I may speak thus—a balance-sheet which, if not certain and inevitable, is at least probable and possible. What can such a patient have? What lesions is he liable to develop some day or other? And these lesions, are they of such a character that it will be urgent or advantageous that they should be treated? What he can have are at first lesions without any real gravity, but which are at least very disagreeable to some, particularly if they are visible; thus he may have cutaneous syphilides of various forms, very annoying syphilides of the mucous membranes, engorgements of the ganglia, alopecia, and onyxis. In the second place, there are more serious lesions, from the fact that some of them are very painful; they are—angina, cephalalgia, various pains with nocturnal exacerbation, insomnia, myalgia, pain in the joints, inflammation of tendons, periostitis, etc. Would not the possible anticipation of such troubles justify the intervention of treatment? But we have really a third order of lesions which are much more serious, and which involve and compromise important organs. Only to cite the most common of this group, we will find—affections of the eye, such as iritis, choroiditis, and retinitis, which are capable of impairing or even extinguishing vision; sarcocele, which may induce disorganization and atrophy of one or both testicles, and thus produce impotence; gummy tumors, which often perforate and destroy the velum palati, and leave a double and revolting infirmity; paralyses of the eye and face; hemiplegia and paraplegia; inflammations of bone, caries, ozœna, flattening and loss of the nose, without speaking of the possibility of hereditary transmission, and of the introduction of syphilis into the family circle. But this is not all yet. If we consult a manual of pathological anatomy, we shall find there described fatal lesions attributable to syphilis alone.

The causes of death in syphilis are many and varied: death by hepatic lesions, cirrhosis, and hepatitis gummosa; death by lesions of the meninges; by cerebral gummata, and syphilitic encephalitis; by lesions of the spinal cord, which are more common than is generally believed; by exostoses of the cranium or vertebræ; by lesions of the kidneys, of the larynx, and of the lungs, and more rarely by lesions of the œsophagus and rectum; death by consumption and progressive cachexia. These are, in short, the possible consequences of syphilis, and such is the perspective offered to a person who contracts this contagion. Dare we call a disease benign which can end thus? Can a disease be called benign which is fraught with such serious accidents, and whose pathological anatomy is so rich and varied? Dare we tell persons afflicted with this disease to leave it untreated, "to let things go," and to wait patiently the possible results of such an infection without warning them of it?

Now suppose for an instant, bearing in mind the tableau which I have carefully sketched, a patient recently contaminated, suppose him on his own account looking the interminable series of accidents of syphilis squarely in the face, and then ask him if there was any reason why he should not try by every means to prepare himself for such events, or whether he preferred to await results. What would be his answer? It would be that he would not under any circumstances think of such a thing as of standing the brunt of such a disease; that it would be foolhardiness not to endeavor to protect himself; and that he was firmly resolved to try every medicine and remedy, and every doctor, in order to be cured; in a word, that he wished *to be treated*, and he would be treated.

Now, we physicians, who are more enlightened than this patient, as to the nature and consequences of syphilis, would we reason otherwise than he did if we were victims of the disease? Certainly not. Simple common-sense, which is greater than all systems and all theories, says that, when a person has syphilis, he should get rid of it, and not allow it to remain in his system.

But here some of our adversaries would interpose by saying: "Undoubtedly you would be right in fearing and wishing to treat your patients, if syphilis invariably ran the course you speak of; but there are two forms of syphilis, the one severe (*forte*) and grave, the other mild (*faible*) and benign. You are right in treating the severer cases, but for the mild cases treatment is wholly unnecessary." But I will reply: Have you the means of pronouncing off-hand (*d'emblée*) upon the nature of a case of syphilis? Do you possess any faculty by which you can form a prognosis as to the future of a given case of syphilis? Can you assert, at the outset, that one patient will suffer severely by syphilis, and that another will be relatively spared? If you are able to make this intuitive (*prévisionnel*) diagnosis upon truly scientific principles, we would consent not to treat those of our patients for whom you pronounced a mild course of syphilis, for we do not treat them for pleasure but for their own benefit, and, before submitting them to the dangers of an expectant treatment, we would demand of you something more than flimsy and theoretical assertions; we would require, in order to be convinced, serious evidence, based upon scientific certainties, and supported by a number of observations, clearly and precisely drawn, otherwise we owe it to our patients and to ourselves to treat them.

Now, do we possess, in the present state of our knowledge, any absolute, or even probable, criterion which enables us to foretell as to the future of syphilis, and which authorizes us in saying positively, such a case will be mild, and such another severe? This is the key-note of the question. Some physicians are led to form a prognosis of syphilis by certain characters of the initial lesion, and of the first eruptions which follow it, and they think they are authorized in drawing the following conclusions: A mild syphilis is one which follows the category of secondary lesions, which begins with a superficial erosion only slightly indurated; and the secondary period of which is ushered in by a mild crop of eruptions, followed by other mild crops, with a long interval between each. On the contrary, a severe syphilis is to be looked for when it has derived its contagion from an initial lesion, which has ulcerated, or is markedly indurated, when the first crop of eruptions is of an ulcerative and suppurative character, and which is followed by other crops of a similar character at short intervals. None of these statements have any real value. None of them warrant the physician in prophesying as to the ultimate evolution of a given case of syphilis, as to whether it will be mild or severe. Under these circumstances would we be warranted in saying to one patient, "It is necessary that you should be treated, as you are in danger;" and to another, "It is not necessary to treat you, as you need fear nothing." To say this would be to prophesy as to the future, and to promise a patient a mild course of syphilis, and to give him a dangerous consolation, and a security of which he would sorely repent some day; and to refrain from treating him, and to leave him to the chances of an unknown future, would be to expose him to serious dangers upon the strength of uncertain statements and visionary assurances. In short, then, we do not possess the elements of certainty, or even of probability, in formulating *ab ovo* a prognosis in a given case of syphilis; and, as such is the case, prudence dictates to us to urge in every case a proper treatment, in order to shorten, if possible, the effects of the diathesis for the present and for the future. This is dictated by common-sense, and is confirmed by observation and experience, in spite of all theories.

The necessity of treatment being thus admitted, its application remains. To what treatment shall we resort? What remedies shall we employ? And especially upon what form of medication shall we place our patients?

According to past and present experience, mercury is the most valuable remedy for syphilis, and therefore claims our attention first. There are, however, two questions to be answered before we prescribe it:

1. Is it deleterious to the patient, and can it in any way become injurious to him?

2. Can it be of benefit to him?

First Question.—Is it deleterious? Is it liable, in any way, to aggravate the condition of the patient, and to add another danger to that of syphilis? This is a very important and very practical question upon which you will be asked daily by your patients, and to which it is necessary to give at once an exact, scientific, and peremptory answer, for mercury has a bad reputation, and is mistrusted by the public. It is a remedy whose name is a terror, and for which all classes of society, the highest and the lowest, entertain an inherent horror. When you prescribe it for a patient, the following stereotyped questions will immediately arise, as they are in everybody's mouth: "But, doctor, you are prescribing mercury for me! So good-by to my teeth and hair, and will you warrant that my bones don't decay? How will you get this mercury out of my system?" I will not, again, endeavor to disprove all the calumnies charged against mercury. It will be remembered that it is said to produce loss of the teeth, alopecia, necrosis, nodes, nervous symptoms of various kinds, anæmia, cachexia, and almost all the lesions due to syphilis, particularly the tertiary lesions. Certain authors have almost come to the conclusion that syphilis does not produce any bad results, but that mercury does. These, however, are only exaggerations and absurdities, to which it is unnecessary to reply, as they have been refuted hundreds of times, and I mention them only to stigmatize them as ridiculous. It is almost unnecessary to say that mercury, administered in a therapeutic dose, as we prudent physicians do nowadays, never produces the results of which it is accused. It is undoubtedly true that even in these doses mercury is liable to produce certain disturbances which are necessary to be understood. We will study these disturbances, and endeavor to determine whether they are of sufficient gravity to contraindicate the use of mercury, or to cause it to be discarded in the treatment of syphilis.

There are three classes of phenomena which are to be feared while administering mercury to syphilitic subjects; they are—

1. Ptyalism (stomatitis and salivation).
2. Gastric and intestinal disturbances.
3. Impairment of the nutrition of the body.

1. *Ptyalism.*—Mercurial stomatitis is a well-known condition, sometimes even produced by inunctions or by the internal administration of mercury, but it is a danger which can be easily avoided. It is only necessary to urge watchfulness as to the action of the remedy, and to give it in such doses and in such forms as not to irritate the teeth, and especially to suspend its use as soon as the mouth becomes slightly sore,

for mercurial stomatitis does not burst out on the patient like a thunder-clap; *it announces itself*, and has a premonitory period of irritation of the gums, in which the physician can take measures to prevent the inflammation. Always suspend the use of mercury as soon as the mouth becomes tender, and administer chlorate of potassa in time, and you will almost to a certainty save the patient from serious consequences. You may judge of this by observation of my service. We always have here more than sixty syphilitic women under a mercurial treatment, and, if mercurial stomatitis were an inevitable result of mercury, it would occur daily in our wards; but it is almost unknown. Undoubtedly you may find, on occasions, some of our patients presenting a slight irritation of the gums; but we interfere in time, and you will *never* see here an example of that frightful stomatitis which ulcerates the gums, causes an incessant flow of saliva, and even threatens to disorganize the maxillæ. In a word, then, with prudence and *watchfulness* (this is the whole secret), we can readily avoid any ptyalizing effects of mercury. Stomatitis is a visionary danger, if we make it our duty to watch our patients carefully; consequently it does not constitute a contraindication to the use of mercury.

2. *Gastric and Intestinal Disturbances.*—It is certain that, in some cases, mercury is badly borne by the stomach and by the intestines. This is to be noticed more frequently in women than in men, especially in women who have a fair skin and who are delicate, lymphatic, and dyspeptic; but this intolerance is very rare, and can be prevented, ameliorated, and even successfully overcome. In order to do this, it is necessary to suit the dose to the degree of tolerance of the stomach, or to combine some modifying agent, such as opium, quinine, or bitter tonic, with the mercurial, or even in some cases to change the form of mercurial. Should it be impossible to administer mercury by the mouth, there remain other methods, such as inunction and subcutaneous injection, by which irritation of the digestive organs is avoided.

The possibility, then, of digestive disturbances is far from being a sufficient reason to contraindicate the use of mercury in syphilis.

3. *Impairment of the Nutrition of the Body.*—It has been said that mercury induces serious nutritive disturbances; that it causes by its poisonous action a chloro-anæmia, and that it defibrinizes the blood. There is a degree of truth in this, as some patients do experience this debilitating (*anémiante*) effect of mercury; but this is especially (I might almost add *only*) when we misuse the remedy, when we give it in excessive doses, or when we use it for too long a time. This, however, is not a danger which suddenly overtakes a patient, for we see its development, and it is easy to avoid it, either in suspending the use of the drug, or by substituting some preparation of iodine, or by combining with it tonic remedies and hygienic measures. Moreover, is not this debilitating influence of mercury exaggerated? We treat here five hundred cases every year with mercurials, and out of this number we certainly do not observe more than an average of five per cent. in whom these disturbances are produced. Nearly all the women in our wards bear mercury admirably; this is the case with the youngest, and even children. Almost all of them take the remedy several weeks continuously, sometimes even for months (with necessary precaution, and short interruptions), without experiencing the least ill effect upon their health; some even grow fat, and present a very enviable appearance. Do we not see that our private patients, who enjoy a better *régime* and hygiene, undergo a mercurial treatment without experiencing the least ill effects, without even *knowing that they take it* (*s'en apercevoir*), as they say, so that they are astonished and have some doubts as to the efficacy of a treatment so mild? According to some observers, mercury possesses tonic properties, and it has been said to cause rabbits to fatten; I cannot vouch for this fact, as I have not seen it; but I can affirm, by experience, that in every case in which it is administered in suitable doses, methodically and watchfully, it is a remedy which is admirably well borne by the economy in the vast majority of cases.

Then this third danger of the disturbances of nutrition by mercury is more theoretical than clinical. This influence shows itself only very rarely, or in cases of careless administration. Again, we find that this is not an argument to oppose to a mercurial treatment, seeing that it really possesses advantages which we will consider farther on.

These, then, are the drawbacks in the use of mercury, and it is only attended with these three inconveniences (for I will not call them dangers), which are generally easy to avoid, to treat, and to shorten. No other accident, I reiterate, results from its administration prudently instituted, accurately measured, and watched with care. Ought we, then, as some of our *confrères* do, to endeavor to exclude mercury from our therapeutics? If that were the case, it would be necessary to give up prescribing almost every remedy, for, when improperly used, it can do mischief. In unskilled or ignorant hands, opium, quinine, arsenic, nitrate of silver, and digitalis, are liable to produce bad results. The same is true of less powerful agents, which, when badly used, are apt to become dangerous. Take, for example, the mild Vichy waters, which, when taken in excess and foolishly, become poisonous. Every year, we hear of persons having died at Vichy, in consequence of drinking the waters without limit and direction.

Let us, then, reason more intelligently than our adversaries, and let us come to this conclusion: If mercury can become dangerous, it is because it is active, and, if it is active, let us know how to profit by its activity, in such a way as to render it useful; for it would be foolish to condemn it from the simple fact that it possesses virtues which we can abuse, or that, when it is badly administered, it is capable of doing harm.

The question of the possible injurious effects of mercury being answered in the negative, let us consider the second point, which is more delicate and more difficult. The question is, Can mercury be useful in syphilis? Every possible and imaginable answer has been made to this question. Some resolutely refuse to accord to mercury the least curative power over syphilis, and say that it aggravates rather than cures it; others go to the other extreme, and cannot indulge sufficiently in praise and in enthusiastic panegyrics upon mercury, which they claim is a specific. They would make you believe that mercury was an antidote created against syphilis. Their confidence in it is unlimited. According to them, it would seem that a pound of this metal, and a quantity of mercurial pills, were sufficient to annihilate syphilis, and forever rid us of this formidable enemy. Let us not be deceived, for these fanatics do more harm to and with mercury than its most violent enemies do; they play the part of the "dangerous friend" of the novelist, who does more harm than a "wise enemy." In order to arrive at the truth of these contradictory opinions, let us endeavor to answer the following questions:

1. Has mercury a real and evident action upon the visible lesions of syphilis?

2. Has mercury an immediate and a remote action upon syphilis? Does it shorten its course, and does it lessen the severity of, or prevent, its later lesions?

First Point.—Let us consider the first question. Suppose we have before us a patient presenting various syphilitic lesions, and that we prescribe mercury for him. Does this remedy exert any manifest influence over these accidents; does it shorten their course, and cause them to disappear sooner than they would if we gave an inert remedy, or no remedy at all? To this, observation replies emphatically in the affirmative, that mercury does exercise an evident influence upon these lesions, renders them milder, and causes them to disappear sooner than they would if left alone, and finally cures them in such a manner as to leave no doubt of its action. The adversaries of mercury say this is a delusion, and that we attribute to the influence of mercury what is really the result of time, and of the natural tendency of the disease. They say that syphilitic lesions are not of long duration, and that they disappear *sponte suâ* after a certain time without the least medication, but upon one person they remain a longer time than upon another; that this is the peculiarity of these lesions, and that we do not know why. But they are certain that mercury has no influence, and that with it or without it they run their natural course. To this it is easy to reply that the syphilitic lesions, particularly the secondary, disappear spontaneously under the influence of time, and the natural course of the disease. We know this from the fact that, in time past, there have been patients who were negligent or unconscious of their disease, and who have not been treated, and upon whom various syphilitic lesions have disappeared spontaneously. But let us establish a comparison. What time is required, on the one hand, for the lesions of a given case of syphilis to disappear spontaneously? And, on the other hand, what is the duration of these lesions when they are treated by mercury? If they will give us their average, we will give them ours.

Now, this comparison has been made seriously, and the result is, as we should expect it to be, in accordance with the experience of our fathers, and with the observation of four centuries. It would be useless to present the full details of the case, so we will take a single illustration: Suppose, by way of comparison of the expectant and mercurial treatments, a well-marked case of lenticular papular syphilide. In what time would it disappear, if left to an expectant treatment? Our opponents would say several months, five to six on the average.

Now, with mercury, this eruption will disappear in from five to six weeks, in two months at the most; and, more than this, it is wonderful to observe that after a fortnight of treatment the influence of the mercury is evident, for the syphilide begins to become paler and to grow smaller. There is another argument in favor of this evident action of mercury, which certainly carries conviction. It sometimes happens that syphilitic lesions have not been diagnosticated as syphilitic, as, for instance, when a scaling syphilide (*syphilide psoriasiforme*) is mistaken for a case of dartrous or arthritic psoriasis, or it happens that a neuralgia due to syphilis is regarded as an ordinary neuralgia. Now, what happens in consequence of such an error? The so-regarded psoriasis is treated for months by arsenic and alkalies, and is not cured; the neuralgia is treated for a long time by opium, quinine, and the bromides, but still it persists. Why is it, then, that this psoriasis is not cured, and that the neuralgia persists, since it is natural, as our opponents say, for every syphilitic manifestation to disappear spontaneously? But this is not all, for, during this time, the physician, having been enlightened by the want of success of his treatment, suspects syphilis as the origin of the troubles, and he prescribes mercury as a touch-stone, when the psoriasis disappears in a few weeks, and the neuralgia is ameliorated in a few days.

What is the secret of this sudden and remarkable cure, of which every physician has seen instances? Time and Nature cannot here be alleged as the cause, for they have had every opportunity before the institution of a mercurial course. Is it simply chance or a coincidence? No one can believe that this chance or coincidence could reproduce itself always under similar conditions. Is it, then, possible to ignore the therapeutic effect, or to deny the manifest curative action of mercury? To deny this would be to place ourselves in defiance of logic and common-sense, and to challenge beforehand and with foregone conclusion the therapeutic action of mercury, than which nothing is more manifest and convincing. The action of mercury, then, upon syphilitic manifestations is an acknowledged scientific fact which has been observed by physicians of all countries and of all ages.

Second Point.—Does mercury exert an *immediate* action upon syphilis? Does it act upon the diathesis in such a manner as to modify it, or to lessen its morbific element, to dilute it, as we say of poisons, so that it mitigates or prevents the later manifestations? Let us put the question practically. A syphilitic patient is troubled with various manifestations, and is treated by mercurials. Now, mercury, we know, will act well upon the visible manifestations, but will it do any thing else? Will it affect the source of the lesions, that is to say, the *disease?* Will it modify the element of the disease, and does it have the power of controlling the diathesis in its evolution, of preventing other lesions, of rendering less severe those which in spite of it will appear; in a word, of *protecting the future after having relieved the present?*

Now, this present and future action of mercury upon syphilis is a point which is most frequently and most warmly contested. A large number of physicians, while they accept the fact of the undoubted action of this remedy upon the lesions of syphilis, refuse to acknowledge its power in modifying the diathesis. "Yes," they say, "mercury does shorten and cure the manifestations of syphilis, but it acts only on the manifestations and does not act upon the disease. It clears off the skin (*il blanchit*), and that is all. The proof," they add, "is, that the manifestations, for which mercury has been given, disappear and are followed by others." In a word, it does not cure, it is simply a palliative of existing lesions, it is not an antidote or a counter-poison of syphilis.

I believe, on the contrary, that the action of mercury is not limited to the lesions, but that it affects the disease. I believe that it, on the one hand, cures the lesions of syphilis, and, on the other, when administered in a manner which I will describe, it exerts upon the whole disease an influence general, prophylactic, and curative. And my opinion on this, so essential and important a point, is not a mere impression or conjecture; it is based upon sound clinical arguments, which I will bring forward.

A priori, I can scarcely conceive how it is possible that mercury, which exercises an undoubted action upon all the lesions and symptoms of every tissue, could possess this action, if it had no influence upon their cause, which is the disease. I can well understand how opium relieves a pain without touching its cause, and how digitalis alleviates affections of the heart without influencing the lesions of the valves; but my mind absolutely fails to understand how a remedy can modify all the results of a poison, and can follow this poison in all the tissues in which it lurks, how it can cure the varied and successive manifestations of a diathesis, without affecting in any degree the poison which is the real cause of all the morbid processes. This, however, may be beyond my comprehension, so I will seek elsewhere for elements of conviction. Now, in clinical observation, we can place side by side cases of syphilis *treated* and cases of syphilis *not treated*. In order, then, to determine the action of mercury, nothing can be more convincing than this comparison, so let us institute it.

In the first place, what do we daily observe upon syphilitic patients who are treated carefully, strictly, and perseveringly? What lesions do they present? How does syphilis show itself upon them?

Syphilis with them is a very slight affair, and I certainly do not exaggerate in stating, after a careful perusal of my notes, that, in ninety-five times out of a hundred *at the least*, if it is treated, it is really mild. Almost all of the patients who take good care of themselves run through their syphilis with very little damage to themselves, and only present a small number of manifestations of no gravity, such as non ulcerating and superficial cutaneous syphilides of the skin, roseolar and papular; syphilides of the mucous membrane showing themselves from time to time in consequence of local irritation (as, for instance, in smoking), adenopathies, a few fugitive pains, temporary thinning of the hair, and other similar slight manifestations. A vast number of my patients have been rid of syphilis at this price, as I have observed years after their infection. I can count by thousands those whom I have seen thus spared by the diathesis in consequence of a treatment properly followed, and there are other physicians who have observed the same in a number of their cases.

The serious and grave accidents of syphilis are very rare in patients who have been treated, and do not occur oftener than five times in a hundred. This is the reason why we see such a difference between cases of syphilis in private practice and those observed in hospitals. Syphilis in hospitals is frightful and disgusting, and the lesions which cause the patients to enter them are hideous and shocking, and often more or less serious. Syphilis of private practice has other features, and shows itself under a much milder aspect. Never do you see, for example, in these cases, those enormous condylomata which you so often see here, which cover the vulva and anus. With the exception of some few cases, you will never see syphilis assume such a wonderful multiplicity of forms and of phenomena as you so frequently see in hospitals. What is the reason of this difference? It is this: that private patients, at the first sight of their disease, consult a physician, for they are intelligent, and understand the necessity of being treated, and *are treated.* Others of them are treated in a desultory manner, just sufficient to merely attenuate the lesions of the diathesis; while, on the contrary, the men and women who frequent the wards of a hospital do not take care of themselves, and only seek our advice at the last moment. They allow the disease to fix itself firmly upon them, and they end by developing the grave form of lesions, which are the combined result of the disease, of negligence, of misery, and of the absolute want of hygiene and treatment.

Having shown the course of a case of syphilis, which has been treated, let us examine the course of a case which is left to its natural evolution. Syphilis becomes serious and formidable, especially when it is not treated. It then *multiplies its blows;* it breaks out in lesions of every class and gravity, occupying the whole body; it causes incurable infirmities, and even compromises life. What we see developed in the secondary period, under these sad conditions, are syphilides of every variety, ulcerating and non-ulcerating; syphilides of the mucous membranes; multiple adenopathies, which under these circumstances sometimes degenerate into buboes which we call strumous; alopecias which render the scalp bald, and sometimes this loss of hair is general throughout the body; pains of every variety, horrible headaches, neuralgias, periostitis, myalgias, and arthralgias; iritis, choroiditis, retinitis, liable to impair vision forever, or to destroy it; sarcocele which may result in atrophy of the testes; nervous affections of various kinds; gastric, intestinal, and nutritive disturbances which may predispose to true cachexia; later, in what we call the tertiary period, we find more destructive syphilides, pustulo-crustaceous, serpiginous, and phagedenic, gummy tumors, followed by ulceration and destruction of organs; exostoses, caries, and necrosis; lesions of the brain and spinal cord, producing hemiplegias, paraplegias, and disturbances of intellect: all of these phenomena producing incurable infirmities, when they do not cause death; visceral lesions of every site, and having an alarming prognosis, not to speak of abortion, miscarriage, and the grave and so often fatal forms of hereditary syphilis.

Is not this contrast between the disastrous results of an expectant treatment, and the usually mild character of a syphilis which is treated, very striking? Still, we too often have the opportunity, here, of drawing such a contrast. Too often do we find poor patients in a very desperate condition, in consequence of their having been treated on the expectant plan. I do not exaggerate when I say that the consequences of this course are *disastrous*—disastrous is the only word with which I can qualify them. A few observations, taken at random from my note-book, are convincing upon this subject:

Here is a young woman who contracted syphilis when eighteen, and she was not treated. At the age of twenty-six she had a gummy tumor of the velum, and she was not treated then. In consequence her velum was destroyed, and you now see her with a double infirmity; a nasal twang of the voice which has become indistinct, almost unintelligible; and regurgitation through the nose of solid and liquid food.

This other woman contracted syphilis from her husband seven years ago. He, wishing to hide his fault, did not have her treated, believing it would amount to nothing. At first some lesions appeared, which were trifling; then a few months ago this horrible, ulcerating, tubercular syphilide appeared, which will leave deep scars, and will disfigure this once pretty woman.

The third example is that of a young actor, who contracted syphilis two years ago. Under the advice of a physician, he was not treated. Then many troubles of a serious nature came on, among which was a hemiplegia evidently due to syphilis. He is left feeble and paralyzed in the right hand, at twenty years of age, and he almost wishes for death.

The fourth case, which was recently in our wards, contracted syphilis three years ago, and was assiduously treated by homœopathy, which means that she was not treated. She had syphilides of the skin and mucous membranes, and pains of every sort, periostitis and other lesions; still she adhered to homœopathy. Finally she developed a double irido-choroiditis, and a gummy tumor of the velum. Her palate was entirely destroyed, one of her eyes has undergone atrophy, and the other has been only slightly improved by a specific treatment instituted too late.

The last example which I will cite, though I have many more, is that of a child of a very respectable family, which was inoculated with syphilis in catheterization of the Eustachian tube. At first the disease was not recognized, and its gravity was not appreciated, and a syphilitic treatment, which was ordered by one of my colleagues, was only pursued for a few weeks. Five years later, a gummy tumor of the velum, and necrosis of the ossa nasi, were developed, and the child lost her velum, and her nose is destroyed and flattened. Such are the features of cases of syphilis not treated. Now, this parallel, it seems to me, furnishes a peremptory answer to the problem under consideration. If such is the effect of mercury upon the diathesis in preventing and in modifying either a part or all of its manifestations, if it acts as a safeguard for the future, while it cures for the present, is it not possible to believe that it has a general influence upon the diseases? It would seem remarkable that mercury, which can change a diathesis so fertile in lesions to an affection attended with a group of lesions comparatively mild, which has the power of attenuating, and of rendering the diathesis mild, and of at least controlling its ulterior evolution, could produce such results without acting upon the disease itself.

We are warranted, then, in concluding that mercury not only cures the lesions, but that it attacks their cause, and, in short, exerts a general curative influence on the disease.

These opinions are, if I may say so, my profession of faith. Studying syphilis for years, I have *learned to fear it*, and, as a consequence, I make it my duty to treat it, and I enforce its treatment. I condemn the expectant treatment, and I cannot speak too severely of it, and I say with conviction that a physician, who, having in his mind such cases as I have described, would submit his patients to the natural evolution of the disease, when he has at hand so powerful a remedy as mercury, this physician incurs one of the gravest of moral responsibilities. I do not prescribe mercury in a routine manner, or according to tradition, but according to my own personal experience, and my conviction from clinical study. I give it to ameliorate the present symptoms, at the same time in *anticipation of the future.* It is not the present or the immediate future of a patient that I fear; it is his remote future, six, ten, twenty, and thirty years hence. In giving mercury I endeavor to save my patients from late visceral lesions, and the object I aim at is to render the diathesis mild at present, in order to shield the patients from danger in the future.

Still, we have not finished with the arguments of the anti-mercurialists. They say: "You prescribe mercury, but it does not prevent relapses; it allows various grave affections to develop at varying periods after its use; and it is not a specific." Let us see what value such objections possess:

1. "Mercury does not prevent relapses." We reply that we undoubtedly do see relapses follow a mercurial course, and have records of such cases in our note-book; but we do not pretend to radically cure syphilis, *to strangle it*, as it were, so that it is forever extinct. On the contrary, we think that, if mercury renders syphilis mild, it attains that great result slowly and progressively; we believe that if we master the diathesis it is only accomplished at the cost of a medication prolonged for a space of time by means of a series of successive treatments. We know full well that a syphilitic patient will be liable to late lesions or relapses, whatever we may do, and we also know from observation quite as well that these relapses are rendered mild in their course, and their dangers are lessened. Relapses following a mercurial treatment are *comparatively mild;* they are, if on the skin, circumscribed and sparse rather than deep, ulcerating, and extensive; they are *aborted relapses*, and unmistakably indicate a progressive attenuation of the diathesis.

Mercury, then, does not suddenly cut short syphilis, it does not prevent relapses, but it progressively renders them less frequent and less severe.

2. Second Objection: "Mercury allows various grave affections to develop at varying periods after its use." This is undoubtedly and unfortunately true, for a mercurial course, long and carefully followed, has not prevented in syphilitics the latter manifestations. But these cases are very rare. I have made careful observations of records for my own instruction, and I can safely say that almost all of the deplorable cases of syphilis are either in persons whose disease was originally wrongly diagnosticated, who have not been treated, or who, believing themselves cured after a few weeks of treatment, have done nothing further. The responsibility of nearly all of the grave cases of syphilis is to be attributed either to the expectant treatment or to incomplete treatment. There are some cases in which mercury fails, but we have never contended for the infallibility of mercury. But this point constitutes the third objection.

3. "Mercury is not a *specific*." Specific is one of those vague words with a double meaning, which every one can define according to his views. If we wish to reserve the title specific for every remedy which exerts peculiar, obscure, and beneficial action upon a disease or symptom, then mercury is a specific. But this is not the sense which is usually meant when it is said that mercury is not a specific for syphilis. In the arguments upon this subject the word specific has become synonymous with infallible, and, in the language of the non-mercurialists, the fact that mercury is not a specific implies that it is not infallible. Let us accept the discussion upon this basis. I repeat that I do not consider mercury infallible, and that I do not give it, expecting it will cure syphilis to a certainty, in every case. On the contrary, I distinctly say that mercury does fail sometimes in rebellious cases. What is most to be feared in this matter is the thoughtless enthusiasm of some of its advocates, who style it an incomparable remedy, a radical antidote, and an unconquerable enemy of syphilis. The real truth is always preferable to such panegyrics, and I repeat that mercury exercises upon the greater number of cases of syphilis a manifest curative action, and that in some cases this action fails or it is insufficient. There certainly are cases in which, though a judicious mercurial course has been persistently followed, new lesions develop, and they pass from the secondary into the tertiary stage, and even threaten life in consequence of visceral lesions; but these are very rare cases. These are my views, and such is the result of my experience as to the degree of confidence which we may place in mercury.

If these rebellious cases were more numerous and should occur frequently, rather than rarely, would that be a reason why we should banish mercury from our therapeutics? Should we renounce it because it only cures, say, ten out of twenty cases? Should we not derive the benefits which it does produce? But what remedy gives better results? Does quinine cure all intermittent fevers? Does copaiba cure all blennorrhagias? Does opium ease every pain? Is there any remedy which is absolutely perfect?

Instead, then, of attacking or rejecting mercury because it does not always cure, let us profit by it as far as we can; let us derive the benefit of its action, however great or small it may be. This is dictated by common-sense.

We are, unfortunately, in the position of having no other remedy as efficacious in secondary syphilis as mercury. We have it and it alone, for iodide of potassium will only serve as an adjuvant in this period. So that, when every thing is taken into consideration, we have no alternative for our choice. This is another excellent reason to justify the employment of mercury.

(TO BE CONTINUED.)

ART. III.—*Report on the Administration of Anæsthetics.* By DAVID WEBSTER, M. D., House-Surgeon to the Manhattan Eye and Ear Hospital, New York.

"How long does it generally take you to put a patient under ether? and in what proportion of cases does vomiting occur during or after its administration?" These are questions which have been repeatedly asked of the attending surgeons of this institution, and which have heretofore been answered somewhat indefinitely, for want of more exact information on the subject.

I was therefore requested to note down, in each case, the length of time from the first inhalation of ether to the commencement of the operation, and the occurrence or absence of vomiting. The number of cases observed and noted having reached one hundred, I have concluded to place the results of my observations before the profession. They are as follows:

Average length of time occupied in producing anæsthesia, 5.84 minutes. The shortest time recorded is one and a half minute, and that was in the case of a child; the longest is twenty minutes; in that case the patient acted badly, the ether having to be "let up" from time to time, in consequence of his becoming asphyxiated.

Vomiting occurred in forty-two per cent. of the cases.

The quantity of ether consumed was not noted; my impression is, that the average was from four to six ounces.

In most cases the patient had eaten no dinner, the inhalations occurring during the afternoon.

As a rule, we advise patients, who are to be operated upon, to abstain from solid food for at least four or five hours before the operation; not because we believe them any the less liable to be nauseated by the ether, but because we wish to avoid the possibility of their being choked to death by such large pieces of unmasticated and undigested meat, potato, pickled cucumber, etc., as we have sometimes seen ejected from the stomachs of patients who had disregarded our injunctions.

Liquid food, on the other hand, such as milk, gruel, etc., may, we think, be partaken of with safety, and will only necessitate the having at hand a large basin in which to catch it when rejected from the stomach.

In giving ether, a newspaper cone, lined with a towel, is always used at this hospital. The cone should be *short*, so as to be as little as possible in the way and in the light. It should be *thick*, so as not to be easily saturated with ether; for, when the paper is wet through, it cannot be made to retain its proper shape, but has an unpleasant tendency to collapse, and asphyxiate the patient by direct contact with his nose and mouth. For obvious reasons, great care should be taken to bury the points of the pins used in constructing the cone.

Three persons are generally necessary in giving ether to an adult—one to manage the cone and the patient's head, and an assistant on each side to hold him in case he struggles violently. This they can best accomplish by placing one hand on his shoulder, while with the other they seize his wrist.

The vast majority of hospital patients resist more or less violently during the stage of excitement, while the reverse seems to be true of the more intelligent and better-trained cases met with in private practice.

The cone should be held a little way from the patient's nose while he takes the first two or three inspirations, so that the ether may be at first largely diluted with air. He will esteem it an act of kindness, and will be less apt to begin to resist as soon as the cone is closely applied. The struggling once commenced, however, more ether should be poured into the cone, the air more perfectly excluded, and the patient hurried through this disagreeable stage as rapidly as possible, His countenance should be narrowly watched, however, and his breathing assiduously attended to. The moment his face commences to assume a purple, or dusky, hue, the ether should be at once removed, and the tongue drawn well forward by means of a tablespoon-handle, or whatever else may be convenient. This is usually sufficient to reëstablish a healthy breathing, and due oxygenation of the blood. But, if it fails, we at once resort to artificial respiration, and dash cold water in his face. I have never seen a case requiring more active measures. I have seen only three or four cases where the patient had to be turned on his side, and have cold water dashed in his face.

In Dr. Agnew's service I have given a mixture consisting of one part of chloroform and four parts of ether to thirteen patients. The average length of time in producing complete anæsthesia was three minutes. The average quantity of the mixture consumed was two ounces. Vomiting occurred in only three cases out of the thirteen. Three patients had eaten their dinners, but in neither of these did vomiting occur.

The last patient to whom we gave this mixture acted very badly under it. He stopped breathing once or twice, and his face exhibited that peculiar dusky hue due to a deficiency of oxygen in the blood. The usual means, however, soon restored the breathing, but vomiting set in, and became so persistent that the doctor was obliged to defer the operation.

The general feeling among the surgeons of this institution seems to be that *chloroform is dangerous*, and that its indiscriminate use as an anæsthetic is unjustifiable.

ART. IV.—*The Pathology of Inflammation.*[1] By THOMAS J. MAYS, M. D., Williamsport, Pa.

ALL diseases to which the human body is liable result from an alteration in the normal nutrition of its histological elements. Where nutrition is impaired, we find, as a consequence, degenerations and infiltrations. Where we have nutrition increased, we find affections of an opposite nature, such as hypertrophies and new formations.

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[1] Read before the Lycoming County (Pa.) Medical Society, May 31, 1872.

But the theme that engages our attention in this paper cannot be classed under either of those heads. Change in the nutrition of a part not alone takes place in inflammation, but it also involves a change in the blood-vessels and circulation. Inflammation takes place as the result of some external injury; this may either be a direct injury, such as chemical or mechanical irritation to the part, or by agents conveyed to it by means of the blood and lymphatics; or indirectly, as in inflammation of the internal organs, arising from a sudden check to the excretions of the skin by exposure to cold.

No subject lies nearer the foundation of all pathological knowledge than that of inflammation.

Before recent experiments, the very offspring and root of all inflammations was supposed to reside in the altered vascular and nervous action.

But, if we take the very simplest case of inflammation in tissue where neither blood-vessels nor nerves exist, we must come to the conclusion that the first step in inflammation does not depend on or exist in either nerves or blood-vessels.

Force and matter are indestructible. Wherever there is a breaking up, or retrograde change in matter, there is a correspondent liberation of force and heat, and a development of motion.

In the steam-engine, the source of energy is the chemical attraction of oxygen for hydrogen and carbon. This force is latent until the particles of matter are brought sufficiently near to enable them to act one on the other, and then there is a retrograde change of the organic fuel into its primary elements, and force is set free, which sets the machine in motion.

In the function of nutrition, we find a process precisely similar taking place in the body, a change of highly-organized material into a lower organism, and, as a result, there is a constant molecular motion. Upon this molecular motion, vital action altogether depends. If it is increased or diminished, disease results. Heat, light, electricity, and chemical action, are forms of molecular motion which, when added to the motion already existing in the tissues, give rise to an increased oxidation of the non-nitrogenous and nitrogenous tissues or substances, even in textures that are wholly devoid of both nerves and blood-vessels; and this changed oxidation determines an altered chemical circulation of lymph or plasma, and an altered nutrition in the inflamed part results.

In all inflammations we have as a necessary accompaniment the development of heat, for the origin of which we can no longer look to an increased vascular action. In the ordinary heat of the body we see it coming from part of the force set free by the action of oxygen in the body. Why not look to the same source in inflammations?

The question of heat plays a most important and essential part in the foundation and development of inflammation. The law of the conservation of energy must be applied here as elsewhere. It is more reasonable to suppose that the heat of inflammation is derived from the same source as the ordinary heat of the body, than to attribute the ordinary heat to chemical action, while the heat in inflammation is supposed to be derived from some other and far less determined source.

All the actions that are going on in a healthy part are intensified, and take on an increased action—a departure from the healthy standard of molecular action, and, as a consequence, there is decreased vitality in the part. In inflammation, there is increased cell-action for normal cell-action, peroxidation for oxidation, increased development and liberation of heat for the normal amount, intensified capillary attraction for ordinary action, increased supply of red globules for a normal supply, an over-development of white blood-globules, increased supply of oxygen for a normal supply, etc.

In the ordinary process of nutrition we have time entering into it as an important and essential element. In inflammation this process is, as it were, compressed into a comparatively smaller space of time, i. e., the work which ought to have been performed in a certain time is done in half or less of that time, and in this manner produces tissue of an inferior and unhealthy quality. The less active the inflammatory process, the more there is a tendency toward the production of normal tissue, and *vice versa*. We see this process going on every day in chronic inflammation, yet no one would say that the two processes, of acute and chronic inflammation, are not of a similar character, only the one is of an active nature, and the other not so active, but of longer duration.

The process of inflammation consists of three consecutive steps, of which the first step is *a change in the molecular motion of a part.* The second is *a change in the blood-vessels and circulation.* The third is *an exudation of liquor sanguinis and fibrine, and migration of white corpuscles.*

First, then, the change in the molecular motion of a part. The single fact that inflammation can be produced in the cornea and cartilage, where neither blood-vessels nor nerves exist, by chemical or mechanical action, is sufficient evidence that inflammation does not depend on nervous and vascular action, but that it may be caused by increased molecular motion; an exaggeration of the ordinary oxidizing action which occurs everywhere in the body, and this increased chemical action sets up secondary mechanical derangements. By increasing the molecular motion in the cells of cartilaginous tissue, by some chemical agent, we produce an increased chemical circulation of lymph, increased consumption of oxygen, increased proliferation of cells, increased liberation of heat and of oxidized products, and finally CO_2 and HO.

This action spreads from cell to cell until it reaches the capillaries. A capillary in ordinary action contains red corpuscles in its centre, loaded with oxygen, while a few white corpuscles sluggishly course along the sides of the vessel, in the liquor sanguinis which moistens the tube around. According to the universal law of diffusion, the oxygen diffuses from the capillary with the lymph into the cells, and thereby active oxidation is kept up outside the wall of the capillaries in and around the different textures.

When the increased or inflammatory action of the molecules reaches the capillaries, the oxygen-bearers are, by the demand for oxygen, attracted in excess, at first producing a more rapid flow through the blood-vessels, and quickly rushing in, so as to cause a heaping up of the blood-globules, causing primarily the enlargement of the vessels, and then obstruction which directly reacts on the heart, increasing the pressure and rate of the blood in all the arteries.

Second Step. Changes in the blood-vessels and circulation. The investigations of Cohnheim, Stricker, Sanderson, and others, prove that the walls of the capillaries are not similar in structure to other blood-vessels, as they were supposed to be, but consist of nothing but protoplasmic cells, situated so as to form tubular channels through which the blood nourishes the tissues. These channels are continuous with the terminal ramifications of the arteries on the one hand, and with the commencing rootlets of veins on the other.

When the increased molecular action reaches the capillaries, the blood-corpuscles coursing along in their natural and usual way are suddenly surprised by a greater demand being made on them for the oxygen which they contain; in consequence of the increased cellular or molecular action of the part, they quickly rush in or are drawn hither by that peculiar capillary attraction, *vis a fronte*, in increased numbers, so as to cause a heaping up of blood-globules. This seems to be the first change that the circulatory fluid undergoes. The superabundance of blood in the capillaries causes an overfulness and distention which finally lead to obstruction or stasis, reacting upon the heart, increasing the rate and pressure of the blood in all the blood-vessels. This pressure is produced in a twofold manner by the two different forces which propel the blood through all its different channels—the *vis a tergo*, or force of the heart, and the *vis a fronte*, or capillary attraction.

There is also an increased production of white corpuscles from the irritation of neighboring lymphatic vessels, and an increase in fibrine. The white corpuscles seem to be increased in large quantities, always having a peculiar affinity for coursing along the wall of the capillary tubes.

After the inflammatory stasis has taken place, pressure and over-distention show themselves very markedly. The pressure destroys the equilibrium that existed before between the inner and outer surface of the capillaries, and causes pain.

The increased supply of blood makes the nerves in and around the obstruction very sensitive.

The changes then that take place in the blood and blood-vessels are: 1. Increased molecular action of the walls of the capillaries. 2. Increased attraction of red corpuscles. 3. Stasis. 4. Reaction of the heart. 5. Increased rate and pressure of the blood. 6. Increased production of white corpuscles and fibrine. 7. Over-pressure and distention.

The third step is *an exudation of white corpuscles, fibrine, and serum.*

The exudations of these different materials are due in a great measure to the over-distention and pressure of the circulatory apparatus. There is no doubt that, as soon as the walls of the capillaries undergo a change, this facilitates the exudation of these different substances, aided and forced by pressure.

The leucocytes which now abound in great numbers, and which sluggishly hang on and are dragged along by the general current of the healthy circulation, become very active, and play an important part in the process of inflammation. They are seen by the microscope to dip into the walls of the canal, and instantly are observed to push their way through on the other side.

Fibrine and serum also exude in large quantities through the walls of the capillaries as the pressure increases, and this fibrinous exudation causes intestinal thickening, and constitutes a much more permanent obstruction than the liquid matter, which can be absorbed rapidly.

Oxidation and pressure cannot be increased in any part of the body without the chemical and formative actions of nutrition being changed at the same time.

Locality has a great influence over the process of inflammation; the hard tissues, such as bone and cartilage, do not yield as readily to it as do the softer tissues. When it occurs in cellular tissue, it is generally of a most active nature. Every cell in the body has the peculiar quality of reproducing itself; and in inflammation, where molecular motion is so much increased, there is also a corresponding number of cells proliferated, which ultimately, with the other effusions, form a subsequent stage of inflammation, that of suppuration.

Clinical Records from Private and Hospital Practice.

I.—*Report of a Case treated in the University of Maryland Hospital, Baltimore. A Supposed Aneurism in the Popliteal Space, without any Characteristic Symptoms by which to establish a Diagnosis.* Service of Prof. J. J. Chisolm.

J. F., aged forty-six, mechanic, tall and thin, was admitted into hospital for surgical treatment, having a large and very painful swelling behind the left knee. He came from a distance, and was accompanied by his attending physician. He gave the following history of the case:

When a young man, twenty-five years of age, he discovered in the ham-region a small lump, the size of the end of the finger, which was hard, painless, and freely movable under the skin. As it caused no inconvenience, its presence was soon overlooked. Year by year it enlarged imperceptibly, so that after twenty years' growing it had filled up the entire hollow behind the knee, causing no discomfort, and in no way interfering with the active duties of a carpenter and house-builder. During these twenty years of growth, the tumor had continued hard, painless, and movable, until restricted from bulk by the ham-string tendons. There had never been throbbing in it, nor had it ever caused pain or swelling in the limb.

Six weeks prior to admission, while in the carpenter-shop, some of the workmen at play during the dinner-hour pushed aside a heavy board, which struck him on the knee, causing so much pain that he had to quit work. From this accident the tumor took on an inflammatory condition, became swollen and very painful, the pain not restricting itself to the popliteal space, but shooting downward to the toes. The physician in attendance, supposing that an abscess was forming, applied poultices for four days, and then plunged a knife one and a half inch into the supposed deeply-seated purulent collection. Only a little blood followed upon the puncture. As the mother of the patient had recently died from cancer, a malignant degeneration of the tumor was now suspected, and the patient was brought to the city for surgical treatment.

Although spare in habit, his general condition was good. When stripped, both inferior extremities, with the exception of the swelling behind the knee, were identical in appearance; long and thin limbs; no œdema nor enlarged superficial vessels appearing about them in proof that the swelling, which was about the size of the fist, had not in any way interfered with the circulation through the left limb. The usual pulsation could be found of equal strength in both legs in the vicinity of the ankle. The tumor, egg-shaped and fist-size, although bound down by the fascia, nevertheless formed quite a prominence in the ham, the ham-string tendons feeling tense as they played over the lateral surfaces of the tumor. When the leg was bent at right angles to the thigh, relaxing all the ham-string muscles, the growth could be seized and sufficient motion imparted to it to insure no periosteal connection. Its outline seemed well defined. It was hard, and not elastic. As the patient had never experienced any throbbing in it from its earliest existence to the present moment, and as after the most careful manipulation no pulsation of any kind could be felt in the swelling, the diagnosis offered was that of an enlarged lymphatic gland, which after many years of slow and painless growth had recently, from injury, taken on an inflammatory condition and possibly degenerative tendency.

As the patient had been brought to the city to have the growth removed, chloroform was administered, and the operation commenced by making an incision five inches long in the axis of the popliteal space, which passed through the skin and fascia, and exposed the tumor. By finger-dissection, always most prudent where growths are to be removed from vascular regions, the sides of the tumor were easily isolated, so that the index-fingers of each hand passed around the growth would touch each other between the tumor and the femur. Contiguous to the bone the femoral artery could be felt, pressure upon it at once stopping pulsation at the ankle. This easy isolation of the sides of the tumor seemed to confirm the diagnosis, and the finger-dissection was continued. In trying to isolate the ends of the ovoidal mass, a stout pedicle, the size of the thumb, was found to extend up the back of the thigh in the midst of the flexor muscles. The dissection in this direction was for the moment suspended, and an attempt was made to liberate the distal end of the growth, when a forked pedicle was also found to issue from this extremity of the tumor. This upper traversing cord, emerging in two footstalks, upon careful examination proved to be the sciatic nerve, with its terminal tibial branches. The removal of the tumor being now quite out of the question, on account of its intimate and serious nerve-connection, an incision was made into its substance to determine its character. Its ovoidal form and easy isolation had been noted, but, in the total absence of all pulsation, any suspicion of aneurism from locality had not been entertained. Upon free incision, the wall of the tumor (for it proved cystic), about one and a half line in thickness, receded from its inherent elasticity, exposing solid contents made up apparently of laminated fibrine, portions of which were taken out for inspection. This seemed to indicate an aneurismal character for the growth, spontaneously cured by being filled with the characteristic laminated contents. As the structure of the tumor was now made out, and all malignant tendency dismissed, it was deemed most prudent for the patient not to empty the sac of its contents, but to close the wound by suture, and leave the growth for removal by absorption. No untoward circumstance occurred during the after-treatment. Two-thirds of the incision from either end healed up by the first intention, leaving the centre a gaping wound, to close up by the slower process of granulation.

It was very interesting to note the process of healing during the month that the patient was kept under observation. The skin, isolated from the tumor, soon became adherent, and from the exposed edges of union granulations sprung up, bordering the orifice made in the tumor by incision through its cystic wall. By degrees these overhanging granulations seemed to take root, as it were, and become adherent to the surface of the laminated fibrine, which composed the bulk of the mass. When he left the hospital to return to his country home, the contents of the tumor could be seen at one point only, where the granulations from the two sides had not yet bridged the gap.

What branch of the popliteal formed this obscure aneurism, with its singular unaneurismal history, was not determined. As the finger could be easily passed between the inner face of the tumor and the femoral artery, in its healthy condition, and as the circulation in the limb was not now, and, to all appearance, never had been disturbed, the ordinary popliteal aneurism could not be made out. The complete fusion of the nerve with the walls of the tumor, spreading out as broad ribbons, and seeming to enter into the very structure of the wall itself, is not the least curious item in this singular case.

Sixteen years since, in removing a very large tumor of the right groin, of rapid growth, with outpulsation, considered malignant, a similar characteristic cyst-wall was exposed, which, when incised, was found filled with fibrinous laminæ. In this case it was deemed prudent to ligate the iliac vessel. The ligature came away on the sixteenth day, and the case was progressing well toward recovery, when symptoms of pyæmia appeared, another surgical case in the ward being similarly attacked, and after ten days the patient was carried off. In the progress of the granulations which sprang up upon the exposed tumor, the superficial fibrinous clots received blood vessels which bled when the probe was used upon these laminated fibrinous masses, and granulations eventually became adherent to them. It seemed as if the newly-vascularized laminated fibrine gave origin to granulations. In this case the tumor was an aneurism of one of the large perforating branches for the profunda femoris, near its origin.

NOTE.—Since the report of the above case had been set in type, and while going through the press, Dr. Chisolm reports the patient returned for further treatment. Two months have elapsed from the period of the exploration. The wound had healed over the tumor, leaving a fistulous opening, from which a very offensive ichorous matter was constantly escaping. This fluid had irritated the skin around the pouting orifice, causing the polished red surface so often seen around fistulous openings connected with caries. The tumor had shrunk to such an extent that the skin covering it—which after the previous operation had to be stretched over the tumor, the sutures cutting out after a very short period—could now be thrown into folds, the cicatrix being firm. The patient complained of severe pain in the leg from the tumor to the toes, especially in the foot, caused by nerve-pressure. The foot and leg exhibited the same normal condition as when first seen. His term of surgical service having expired, the case fell into the hands of his successor, who, not having seen it before, considered the fistulous opening and polished skin evidences of malignancy, and amputated the limb. The tumor was examined, by free incision through it. It consisted of a very firm, thick, fibrous sac, which inflammation, owing to the former partial isolation of the tumor, had much thickened from without, and had matted to the ham-tissues. The centre of the solid contents had undergone physical changes from two months' exposure, with surface liquefaction, and resembled the ragged appearance seen in cutting into a cancer, the centre of which had softened with open ulcer. But, with this appearance, analogy ceased, for the solid contents, as a firm mass with polished outer surface, were easily turned out of the sac, leaving the inner surface of the fibrinous sac also polished, there being no intimate adhesions between them.

Dr. Chisolm procured a portion of this solid contents and submitted it to a skilled microscopist, who reported hematin in quantity, also narrow spindle-cells, such as are seen in inflammatory new formations. After a most careful search, no cancer-elements were found.

II.—*Simple Dislocation backward of the Fifth Metacarpal Bone at its Carpo-metacarpal Articulation; Reduction.*

By L. D. Mason, M. D., Brooklyn, L. I.

J. B., Irish, aged thirty-five, sailor, applied for treatment at the out-door department of the Long Island College Hospital, on the 15th of June last, at 2 P. M. He gave the following history: About 6 P. M. the day previous, while working on a dredging-machine, he attempted to disengage a rope which had become kinked, in passing over a "winch," turned by steam-power; while in the act of so doing, his hand was caught by the rope, and forcibly flexed upon the wrist, and at the same time the ulnar side was twisted inward. The engine was promptly stopped, and, on freeing his hand, he discovered that his fingers were severely lacerated, and that he had "started a bone on the back of his hand." On examination, a distinct bony elevation was observed on the back of the hand, in a situation corresponding to the carpo-metacarpal joint of the fifth metacarpal bone. It was exceedingly tender when pressed upon. Careful examination eliminated fracture of metacarpal bone at this point, also displacement of any carpal bone. There was very slight œdema at the seat of injury, so that the sharp edge of the articular surface of the proximal end of the metacarpal bone could be readily traced through the skin.

Forcible flexion of the metacarpal bone increased the deformity. Firm pressure, directly downward, did not tend in any way to lessen it.

A diagnosis of dislocation backward of the proximal end of the fifth metacarpal bone was thus made out.

This diagnosis was confirmed by Dr. R. Hesse, Dr. Charles P. Donelson, and several members of the graduating class then present.

The patient was now anæsthetized, and reduction easily effected by fixing the thumbs firmly against the articulating surface, and pressing steadily forward, at the same time extension being made at the distal extremity of the bone.

The first effort proved successful, the bone sliding readily into place, with a dull sound, and a sensation not unlike that which accompanies the breaking down of a simple ganglion by forcible pressure with the thumbs.

After reduction, a compress and roller-bandage were applied. At the end of three days these were removed, the patient expressing himself as feeling easier than he did before reduction was effected. He has been seen several times since, but there has not been any tendency toward reproduction of the dislocation.

III.—*A Case of Scalping.* By THEODORE F. BREEK, M. D.

[Boston Medical and Surgical Journal, July 4, 1872.]

August 15, 1871.—Miss E. W., aged twenty-one, an employée of a Springfield manufacturing company, while reaching to take something from a shelf above her head, caught the net covering her hair, and her hair also, on a revolving shaft two inches in diameter, which was making one hundred and twenty-eight revolutions per minute. Her entire scalp was torn off, with the subjacent muscular and other tissues, down to the pericranium, from the fifth cervical vertebra posteriorly, to the bridge of the nose anteriorly, and laterally from ear to ear, including the skin of both eyelids and a piece of skin two inches square from the left cheek.

I reached the factory some thirty minutes after the accident, and found the patient sitting on the floor, with her hands clasped around her knees, and apparently very calm and collected. There was very little hæmorrhage; her pulse was 80, about normal, and her respiration regular and not rapid. She was immediately removed to her home, and the wound dressed with a weak solution of carbolic acid. No attempt was made to replace the scalp, as it was torn into shapeless fragments.

During the next twenty-four hours she was given milk and stimulants freely, and morphiæ sulph. gr. ⅛ every three hours.

August 16th.—Patient very comfortable; complains of no pain. Pulse 80, full and regular. Treatment continued.

August 17th.—There has been considerable hæmorrhage the past twelve hours, which, upon removing the dressings, was found to come from the supra-orbital artery. It was checked by pressure and the application of ferri persulph. Patient somewhat excited. Pulse 110.

August 18th.—Passed a comfortable night, and is much refreshed. Pulse 105, strong.

August 19th.—Upon removing the dressings, suppuration was found to have begun, and the wound was dressed with ungt. creasoti. Bowels freely moved by enema. Opiate discontinued. During the next two weeks the patient's condition steadily improved. She then had severe inflammation of the left eye, resulting in its destruction. The origin of this trouble was a fragment of a lead-pencil which she had behind her ear at the time of the accident.

It is now nine months since the accident, and the process of restoration has been very slow. At times the skin forms very rapidly, then what has already formed will slough off, leaving the whole wound an ulcerating surface.

Various applications have been made, but with little benefit. The patient's health is excellent, she being able to go about and attend to the various duties of life. The question is, Will skin ever form over the entire surface of the wound?

About the time of this accident, several cases very similar occurred; one in particular, at the Elgin (Ill.) watch-factory. I should judge, from the description which I received from the attending surgeon, that it was identical with the present case. There the scalp was replaced, but with no success, as it sloughed off on the fourth or fifth day.

I have advised the patient to submit to Reverdin's operation of skin-transplantation, but she is very unwilling to allow any surgical interference.]

IV.—*Case of Sunstroke, with Extraordinary Temperature.* Bellevue Hospital. Service of A. Flint, Jr., M. D. Reported by Dr. Katzabach.

July 2d.—Thomas Kilkenny, aged thirty-six, married, was taken from the front platform of a Second-Avenue car, and brought to Ward 32 by a policeman at 2 p. m.

On admission, patient was in a state of profound coma. Was stripped and laid on the floor near a window, with rubber-cloth under him. The heart was acting violently, and there was marked throbbing of all the superficial arteries, so that with each pulsation the whole body appeared to be shaken. The face was congested, pupils very much contracted. Chest not examined. Pulse, 150; respirations, 22, quiet; temperature, 110½°. Was at once covered with a wet sheet, and this was ordered to be sprinkled with cold water. 2.35 p. m.: pulse, 150; respirations, 36; temperature, 110°. Bowels not having moved, the following enema was ordered:

℞. Ol. tiglii, gtt. iij;
Ol. terebinthinæ, ℥j;
Ol. ricini, ℥ij.
M.

3.25 p. m.: pulse, 168; respirations, 40; temperature, 108°. Gave spiritus chloroformi, ʒss, by the mouth. Enema has acted, producing copious yellow fluid evacuations. Breathing is becoming stertorous. 4.00 p. m.: pulse, 158; respirations, 38; temperature, 107½°. Breathing more noisy; expiration moaning. Sprinkling has been kept up since admission. 4.30 p. m.: pulse, 158; respirations, 48; temperature, 104¾°. Patient is becoming cyanosed. Sheet ordered off, and dry cups to chest. 5 p. m.: cyanosis much diminished; pulse, 130; respiration and temperature not taken. Ordered to be covered again with the wet sheet and sprinkled. 5.30 p. m.: pulse, 126; respirations, 28; temperature, 102°. Has been vomiting. Wet sheet stopped, and cold sponging ordered. 7.15 p. m.: pulse, 120; respirations, 24; temperature, 101⅛°. Has been struggling; breathing noisy; pupils widely dilated, and do not respond; face congested. 9 P. M.: pulse being weak, tr. digitalis, ♏x, given subcutaneously. Appears to notice what is going on about him. Sponging stopped. 9.20 P. M.: pulse stronger and fuller. 9.45 P. M.: breathing quiet; temperature, 100¾°. There is contraction of the forearms, with marked rigidity. Subcutaneous injection of tr. digitalis repeated. 10 P. M.: pulse irregular; sleeping. 12 midnight: pulse, 86, small and feeble; respirations, 48, superficial; temperature, 101¼°. Patient is wide awake, and appears much more rational. His head being off the pillow, he attempted to replace it when told to. Gave sol. morph. sulph. (Mag.) ♏vj, subcutaneously.

July 3d.—9 A. M.: pulse, 92; respirations, 18; temperature, 100½°. Sleeping quietly; pupils somewhat contracted. Ordered wine, and diet of milk, eggs, and beef-tea. 10 P. M.: has had copious liquid discharges from the bowels since last note. Ordered opium and bismuth-powder. Is still stupid; speaks very little; apparently very weak; gets sherry wine ℥j every two hours. 4.30 P. M.: pulse, 84; respirations, 20; temperature, 100½°. 10.30 P. M.: discharges from bowels have recurred; passes his urine voluntarily and in large quantities, calling for duck.

July 4th.—9 A. M.: pulse, 88; respirations, 22; temperature, 101°. Stimulants continued. Patient's condition improving. 4.30 P. M.: pulse, 92; respirations, 28; temperature, 101½°.

July 5th.—9 A. M.: pulse, 84; respirations, 30; temperature, 101°. *Chest* examined. Posteriorly: percussion-note clear; respiration rough in character and on the right side, accompanied by a few moist *râles*. Anteriorly: percussion-note good; respiration feeble, otherwise, good. *Heart*-sounds good. Has vomited this morning. Ordered quin. sulph. gr. x to be given in the twenty-four hours.

V.—*Chloral Hydrate in Pertussis.* By P. Brynberg Porter, M. D., Attending Physician to the New York Free Dispensary for Sick Children, and for Diseases of Women at the Northeastern Dispensary.

Nearly all my cases of whooping-cough having occurred in dispensary practice, which is proverbially unsatisfactory on account of the difficulty of obtaining results, the following are all that I have been able to observe up to the termination of the affection.

I have prescribed chloral hydrate in quite a large number of cases besides these; but the patients either reported but once, or neglected to return at all, so that I could preserve no record of them.

I will state, however, that in not a single case, as far as could be ascertained, was the hydrate exhibited without its being followed by an alleviation of the symptoms.

Thus, in a patient suffering from a very serious attack, complicated by severe bronchitis, I found a very marked improvement at the end of three days from the time I had prescribed it. In this case, however, syrup of ipecacuanha and syrup of squill, with turpentine stupes externally, were employed for the relief of the bronchitis.

In no case was any injurious effect observed to be produced by the chloral.

Case I. *September* 28, 1871.—David R., aged two and a half years. Has been sick for over a week, and the characteristic paroxysms have already commenced. Ordered tincture of belladonna and "brown mixture."

October 5th.—No better. Seems to have derived no benefit from the treatment. Ordered one and a half grain chloral hydrate, in syrup-and-water, every three hours.

7th.—Marked improvement. Paroxysms much less frequent as well as less severe. Ordered the same to be continued.

19th.—Got so much better by the time the last bottle was used that the mother thought it was not worth while to return for any more. The cough is worse again now. Ordered the chloral in the same dose.

26th.—Much improved since the 19th. The paroxysms occur at rarer intervals, and seem to be considerably shortened in duration. Ordered the same continued.

December 12th.—Continued steadily to improve after the last date. Cough all gone. The last vestiges of the "whoop" disappeared at least three weeks ago.

In the remaining cases the hydrate was the only agent employed. The next three all belonged to the same family. The happy effect of the remedy seems especially well shown in Cases III. and IV.

Case II. *December* 30, 1871.—Sarah S., aged six years. The cough commenced more than two weeks ago, and has now reached the paroxysmal stage. Ordered three grains chloral every three hours.

January 6, 1872.—Marked improvement. Paroxysms not so frequent and do not last so long. Expectorates more freely. Ordered the same continued.

13th.—Improvement still more marked. Does not get black and blue in the face now, as she did for a time. Sleeps much better at night. Her appetite, which has hitherto been poor, is now excellent. Ordered the same continued.

27th.—Still continues to improve. Ordered the same continued.

Case III. *January* 6, 1872.—George S., aged nine years. Had been treated previously for tuberculosis, and had greatly improved. Pertussis commenced three weeks ago, and the disease is now fully developed, with extremely severe paroxysms. Ordered five grains chloral every three hours.

13th.—Considerable improvement. Paroxysms less frequent and severe. Ordered the same continued.

23d.—Very marked improvement for some days after the last date, when, the medicine having given out, the paroxysms began to increase in violence and frequency, and have become quite bad again now. Ordered the same continued.

Case IV. *January* 6, 1872.—Emma S., aged eleven years. Commenced coughing more than two weeks ago. Apparently not a very severe case. Ordered five grains chloral every three hours.

13th.—So much better that she scarcely coughs at all. Ordered the same continued.

28th.—Seemed almost entirely well while the medicine lasted, but when it was gone the paroxysms returned with great frequency. They have also increased vastly in violence, so that they are now much worse than at any time before. Ordered the same continued. I learned afterward from the mother that in all three of these children the disease subsided very rapidly after the last date, and so completely that it was not necessary for either of them to take any more medicine after the quantity ordered at that time was exhausted.

Case V. *June* 17, 1872.—Annie D., aged twenty-one months. Cough has lasted three weeks. Commenced to whoop one week ago. Paroxysms very severe now. Ordered two grains of chloral every three hours.

19th.—Paroxysms less frequent, but equally severe. Increased chloral to two and a half grains.

24th.—Remarkable improvement. Has no severe paroxysms whatever. Ordered the same continued.

26th.—The cough "broken." Does not whoop at all. The child practically well.

Case VI. *June 24th.*—Mary R., aged ten months. Has had pertussis five weeks. Paroxysms of alarming violence. Ordered one grain chloral every three hours.

26th.—Improvement almost magical. Paroxysms very much less frequent and severe. The little patient much easier and better in every respect, and the mother delighted. Increased chloral to one and a quarter grain.

28th.—Improvement still continues. Scarcely suffers at all from the paroxysms. Increased chloral to one and a half grain.

July 3d.—The whoop entirely disappeared some days ago. Has no paroxysms whatever. Doing capitally in every respect. A little trace of bronchitis all that is left.

As the result of my experience, I would state, therefore, that I am fully convinced of the marked effect of chloral hydrate in alleviating the symptoms of pertussis, and that there seems to be some evidence (though my number of cases is certainly very limited) to show that it has a positive effect in cutting short the disorder. It is the only remedy I have employed in this affection at the Children's Dispensary for some time.

Bibliographical and Literary Notes.

ART. I.—*Electricity in its Relations to Practical Medicine.* By Dr. MORITZ MEYER, Royal Counsellor of Health, etc. Second revised and corrected American edition. Translated from the third German edition, with Notes and Additions, by William A. Hammond, M. D. New York: D. Appleton & Co., 1871. 8vo, pp. xiv.–506.

THE fact that a second edition of this work has been called for, shows that the interest among physicians in electricity is increasing, especially considering that, since the first edition was published, several other works have been issued, here and abroad. Of the plan and execution of the work there is no need to say any thing, as they are not altered, and the book has been two years before the profession; indeed, the exhaustion of the first edition shows that it is appreciated as a valuable guide in electro-therapeutics. In many respects the present edition is an improvement, errors having been corrected. The prefaces to the first and second German editions have been omitted. An appendix has been added, describing several new apparatus made by the Galvano-Faradaic Manufacturing Company, which possess several advantages over many other machines. This electro-magnetic machine is so constructed as to be portable without danger of slopping over; also the rapidity of the interruptions can be varied by a very simple contrivance. Their portable galvanic battery seems to be essentially Stöhrer's, with the cells covered by a rubber-cap to prevent slopping. Their permanent galvanic battery is a modification of Daniel's elements with Remak's apparatus, having, however, in addition two pillars filled with water, by passing through which the current may be weakened; and also a clock-work mechanism to rapidly interrupt the current. The point in which this machine is faulty is, that the cells can be increased by twos up to ten, then there must be a break; twelve cells can be used only by going back to a smaller number and then jumping ten cells; so of any number between twenty, and thirty, or between thirty and forty, or forty and fifty. The American galvano-caustic battery scarcely deserves the title American, as it is a double Stöhrer's galvano-caustic battery, differing from that only by having four cells instead of two. By a singular oversight the nature of the exciting fluid is not given.

This appendix has rather too much the appearance of an advertisement, as though it was written to form part of a catalogue. Improvements made by other manufacturers are omitted.

ART. II.—*Lithotomy and Lithotrity.* Illustrated by Cases in the Practice of GURDON BUCK, M. D., Visiting Surgeon to the New York Hospital and Presbyterian Hospital; Consulting Surgeon to Roosevelt Hospital and St. Luke's Hospital. New York: Wm. Wood & Co., 1872.

THIS pamphlet is a reprint of the author's paper on these subjects, which appeared in the Transactions of the Medical Society of the State of New York for 1870, together with a supplementary report of some six new cases. Dr. Buck thus gives to the profession the carefully-recorded histories of fifty cases of vesical calculus which he has operated upon either by lithotomy or lithotrity. In relation to the choice of these operations for the removal of stone, we believe the experience of Dr. Buck, as given in this paper, is in accord with that of the majority of surgeons at the present day.

ART. III.—*The Correct Principles of Treatment for Angular Curvature of the Spine.* By BENJAMIN LEE, A. M., M. D. Philadelphia: J. B. Lippincott & Co., 1872.

WE have in this little work simply the essay of Dr. Lee upon antero-posterior support, which has already been placed before the profession in the "Transactions of the American Medical Association for 1866," together with his paper on "Modified Suspension," extracted from the "Transactions of the Medical Society of Pennsylvania for 1870." In both of these papers will be found very clearly set forth the principles which Dr. Lee advocates in the treatment of this disease, and we believe they are the same as those approved by the best authorities in this city.

ANNOUNCEMENTS.—Lindsay & Blakiston have in preparation a reprint of the sixth edition of "Aitkin's Science and Practice of Medicine," edited by Meredith Clymer, M. D.

BOOKS AND PAMPHLETS RECEIVED.—A System of Surgery; Pathological, Diagnostic, Therapeutic, and Operative. By Samuel D. Gross, M. D., LL. D., D. C. L. Oxon., Professor of Surgery in the Jefferson Medical College of Philadelphia, etc., etc. Illustrated by upward of Fourteen Hundred Engravings. Fifth edition, greatly enlarged and thoroughly revised. In two volumes. Philadelphia: Henry C. Lea, 1872.

On Winter Cough, Catarrh, Bronchitis, Emphysema, Asthma: A Course of Lectures delivered at the Royal Hospital for Diseases of the Chest. By Horace Dobell, M. D., Senior Physician to the Hospital. New and enlarged edition, with colored Plates. London: J. & A. Churchill, 1872.

Autumnal Catarrh (Hay Fever). With Three Maps. By Morill Wyman, M. D. Late Hersey Professor Adjunct of the Theory and Practice of Medicine in Harvard University. New York: Hurd & Houghton. Cambridge: the Riverside Press, 1872.

The Medical Register of New York and Vicinity, for the Year commencing June 1, 1872. Published under the Supervision of the New York Medico-Historical Society. A. E. M. Purdy, M. D., Editor. Vol. X. New York: William Wood & Co., 1872.

The Chronic Diseases of Women, with Special Regard to their Treatment in Creuznach. By Louis Michels, M. D. Second, improved edition. Berlin: Mitscher & Röstell. London: Trübner & Co., 1872.

Minutes of the Twenty-third Annual Meeting of the American Medical Association, held in the City of Philadelphia, May, 1872. Philadelphia: William B. Atkinson, M. D., 1400 Pine Street, 1872.

Ninth Annual Report of the New York Society for the Relief of the Ruptured and Crippled, May, 1872. New York: Poole & Maclauchlan, pp. 53.

A Plea for the Antiphlogistic Treatment of Disease. By Edward Montgomery, M. D. Reprinted from the St. Louis *Medical Archives*, pp. 20.

Sixth Annual Report of the American Society for the Prevention of Cruelty to Animals, 1872.

Reports on the Progress of Medicine.

REPORT ON NEUROLOGICAL MEDICINE.

BY S. G. WEBBER, M. D., BOSTON.

1.—*Exposure and Stretching of the Four Lower Cervical Nerves at the Spinal Column.* By Prof. V. NUSSBAUM. Ærztl. Int. Bl., 1872, 9. [All. Med. Cent. Zeitung, 1872, 24.]

THE patient, Rudolf Hailer, soldier, had been wounded in the left elbow and in the neck; at the latter place an abscess formed, which healed. Subsequently there occurred contraction of the left pectoralis major and minor, and of all the flexors of the left arm and forearm and hand. The contraction was so powerful that it was not possible to extend the contracted limbs. Sensibility was also diminished, but not entirely lost. When under the influence of chloroform, the contraction ceased, but before recovery of consciousness it returned so powerfully, that the splints and bandages had to be removed or the limb would have been seriously injured. All the various medicinal agents, electricity, and bathing, produced no relief.

Having seen a case in which, during resection of the elbow, traction on the ulnar nerve seemed to be of use in relieving painful contraction of the fourth and fifth fingers, the author determined to expose the four lower cervical nerves and put them on a stretch, at their exit from the spinal foramina, so as to break up any adhesions which might act as a source of irritation.

February 15, 1872.—The patient was chloroformed; a longitudinal incision was made over the ulnar nerve at the elbow; the nerve was removed from its bed in the groove of the bone and gently stretched; it was then replaced, and the wound cleaned and closed. A second incision was made in the axilla; the nerves were separated from their surrounding cellular tissue, and gently stretched; stretching of the median, radial, and ulnar, caused contraction of the corresponding muscles. The parts were adjusted and the wound closed. A transverse incision three inches long was then made over the larger arch of the left clavicle, the platysma myoides separated, and the inferior cervical nerves, lying partly in front and partly behind the sub-clavicular artery, were exposed by forceps, the nerves were raised on the fingers and stretched thereby; each nerve was then followed with the index-fingers to the vertebral column, which was not difficult; at their exit they were passed upward and downward, right and left; were seized and gently drawn upon in a direction as if one would pull them out of the cord. During this manipulation there was spasm in the muscles supplied by these nerves. The wound was then cleaned and secured; two small cutaneous vessels were tied.

The patient came slowly out of the narcosis, and the fearful tonic cramp, which had formerly returned before consciousness, did not reappear. The forearm and fingers could be extended and flexed. The parts where sensation was diminished had nearly normal sensibility. The wound in the neck was opened, to allow exit for the secretions, and cleansed with carbolic acid. The healing of the wounds occurred without accident. From day to day the muscles became softer, and reacted better to the impulse of the will, and sensation improved.

2.—*Contributions to the Physiology of the Corpora Quadrigemina.* BY PH. KNOLL. From Eckhardt's Beitr. [Centralblatt für d. Med. Wissenschaften, 1872, 17.]

The author found that division of the optic nerve between the eye and the chiasm produced dilatation of the pupil and paralysis on the corresponding side, the opposite side not being affected. But division between the chiasma and the brain produced these effects on the opposite side, showing that in the chiasma there is a complete decussation of the fibres which influence the oculo-motor. The action of the oculo-motor nerve upon the iris is purely reflex, since, after the division of the optic, subsequent division of the oculo-motor has no influence over the pupil.

Injury of the optic thalamus or of the corpora quadrigemina produced no effect on the pupil so long as fibres of the optic tract were not touched. If these latter fibres were injured, the iris on the opposite side was paralyzed. Injury to the corpora quadrigemina caused no disturbance of motor power.

Irritation of the anterior corpora quadrigemina produced dilatation of the pupils in both eyes, which ceased on division of the cervical sympathetic.

3.—*Post-mortem Appearances in a Case of Recovery from Paraplegia.* By MORIZ BENEDIKT. [All. Wien. Med. Zeitung, 1872, 1.]

The patient, a woman, thirty-three years old, was received November 11, 1870. She had never been strong, had gastric cramps, pharyngismus, cold feet, sense of heat, with redness in breast and neck. About two and a half years previously she had suffered from diarrhœa, and since the last attack, two and a quarter years previously, she had suffered as on admission. She could neither stand, nor walk, nor sit; could not raise her head, and had to be fed; the voice was completely aphonic, and the slight whispering required great exertion. There were constipation, gastric cramp, and pharyngismus, and at times vomiting. There was no psychical disturbance, no disturbance of sensibility, no cramps other than those mentioned.

The galvanic reaction was excessive. Voluntary motion, while in bed, slow, imperfect, and of short duration. By spinal-cord-nerve and spinal-cord-muscle current the patient recovered power over her muscles, so that by December 19th she could be considered cured. She became sick, and on December 26th died with typhoid symptoms.

The next day, at the autopsy, were found the ordinary pathological changes of typhoid fever, the hymen perfect, the uterus not developed. The central nervous system and its envelopes seemed normal. There were no granular fatty cells. After hardening in bichromate of potassa, there was seen nowhere a normal (?) coloring, nor any change on the application of solution of carmine. On microscopic examination, the central canal was filled with nuclei, which were also found in the gray commissure and also in the anterior and posterior cornua. The anterior columns were united across the anterior fissure by bridges of connective tissue, the broad basis on the left side, the narrower apex on the right. The principal changes were found in the anterior columns; they consisted: first, in large spots which were colored red, and contained no nerve-fibres or only very few—these were found chiefly in the left column, at the base of the above-mentioned bridge; secondly, the transverse sections of the nerve-fibres were separated from each other by wide spaces of connective tissue; thirdly, there were flakes which had a gray color in the red-colored preparations, and were chiefly amorphous or granular; in these were found axis cylinders colored red. There were also oil-like globules scattered over the anterior columns, sometimes in connection with nerve-fibres, sometimes with the connective tissue, perhaps colloid degeneration. A similar though less-marked change was found in the lateral columns, and sometimes in the posterior. The gray substance seemed normal. The hypoglossal, vagus, facial, and abducens nerves showed changes, chiefly increase of nuclei around the vessels, and also the oil-like globules such as were seen in the spinal cord. The trigeminus seemed healthy.

It must be remarked that this was a case which ought to be included among hysterical paralyses. The author refers it to a swelling of the spinal cord. He finds it exceedingly difficult or impossible to distinguish clinically between hysterical paralyses and those caused by myelitis, and this has convinced him that swelling lies at the foundation of all hysterical paraplegias, as well as of the combination of hysterical symptoms resulting from a central cause, and this swelling may also lead to sclerosis.

4.—*Means of arresting Epileptic Attacks and Convulsions caused by Strychnia and Loss of Blood.* By Brown-Séquard. [Arch. de Physiol. Norm. et Path., 1872, 2.]

The author arrests the attacks of epilepsy excited in Guinea-pigs by irritating the epileptogenous zone, by irritating the mucous membrane of the pharynx (perhaps especially or solely of the larynx), by injecting a current of carbonic acid. The current of gas must be expelled forcibly from a tube carried far back in the mouth.

Rosenthal's (of Berlin) experiment in arresting the convulsions caused by strychnia, by pulmonary insufflation, Brown-Séquard thinks was successful on account of the irritation of the branches of the vagus in the bronchi, and of the phrenic and other nerves of the diaphragm, and not on account of the superoxygenation of the blood.

He concludes that carbonic acid is a very powerful excitant of the nervous ramifications of the vagus, and perhaps of other nerves of the pharynx, larynx, and bronchi, and that irritation of these will produce the arrest, suspension, inhibition, or, if preferred, the cessation of activity of the parts of the nervous centres which are active in the production of convulsions due to epilepsy, poisoning by strychnia, or the rapid and considerable loss of blood.

5.—*An Interesting Case of Abscess of the Cerebral Cortical Substance.* By Dr. Eduard Hitzig. [Archiv f. Psychiatrie und Nervenkrankh., iii., 3.]

This was the case of a French soldier who was wounded at Orleans on the right side of the head. The wound did not do well, gangrene set in, and in about two months he died. The principal cerebral symptoms were, first, clonic spasm, without loss of consciousness of the muscles of the left side of the face and of the tongue. Immediately after this attack of spasm, which lasted about five minutes, there was a temporary but almost complete paralysis of the left facial muscles, and of the left side of the tongue. During the attack, the arterial tension was greater on the left; after the attack, it was greater on the right. Afterward there was spasm of one portion, and partial paralysis of another portion of the left side of the face; also subsequently the spasms extended to the left arm and left side of the thorax, and finally to the right arm.

The author considers that the restriction of all paralytic or spasmodic symptoms at first to a certain set of muscles—those innervated by the left facial and hypoglossal nerves—shows that the lesion must first have implicated the centres of innervation of these nerves. The greatest degeneration of cerebral substance was found at a point just anterior to the fissure of Roland. Here was a small abscess, 1½ to 2 centimetres in diameter, and scarcely so deep, its upper edge about 6½ centimetres from the median line, its posterior edge 2½ centimetres anterior to the middle portion of the fissure of Sylvius.

Comparing this location with the centre for the facial in dogs, found by experimenting, there is seen to be a marked coincidence, except that the latter is slightly higher, which might be expected from the difference in the development of the human and canine brains. To prove the identity of the two centres, however, more data are needed.

6.—*Epileptic Attacks after crushing the Sciatic Nerve.* By Billroth. V. Langenbeck's Archiv, xiii., 2. [Berliner Klin. Wochenschrift, 1872, 10.]

A young man fell from a ladder, striking his right buttock against the corner of a table. From the severe pain, and the impossibility of active motion in the right leg, also from the presence of blood in the urine, and a painful swelling between the sacrum and the tuber ischii, the diagnosis made was fissure of the pelvis, and irritation of the sciatic nerve at the tuber ischii. Besides severe pain, there were cramp-like contractions in the affected side from the beginning, which in the ninth week became general, with at times loss of consciousness. Friction over certain spinous processes, pressure on the sciatic, the calf, and other regions, excited the attacks. The nerve was exposed, to remove any cause of irritation. Nothing abnormal was found in the nerve or its vicinity. The motion, however, subsequently returned, and the attack ceased gradually.

7.—*Structure of the Gray Substance of the Human Cerebrum.* By Prof. Gerlach. [Centralblatt für die Med. Wissensch., 1872, 18.]

Gerlach colored his specimens with gold, and found that, besides the medullary nerve-fibres entering the gray substance from the white radiating in bundles toward the circumference, there are numerous nerve-fibres, likewise containing medullary substance, running horizontally between these, where also is to be found the principal location of the ganglion-cells.

2. In the meshes of this great net-work of medullary fibres lie, besides the ganglion-cells, a second extremely fine net-work of fibres no longer medullary. The finest processes of the nerve-cells contribute to form this net-work, and from it arise fibres which, becoming thicker, soon are surrounded with medullary substance, there being no finely-granular mass between the two, as Rindfleisch believes.

3. On the nerve-cells are found the processes of Deiters, which, without dividing, pass directly into the axis cylinder of a medullary nerve-fibre. These belong to the radiating bundles of fibres.

4. In the gray substance of the convolutions of the human cerebrum are then two sorts of origin for the medullary nerve-fibres; one directly from the cells, the other from the net-work.

8.—*Termination of Nerves in the Cortical Cerebral Substance.* E. Rindfleisch. M. Schultze's Arch f. Mikr. Anat., viii. [Centralblatt f. d. Med. Wissensch., 1872, 18.]

Small pieces of cortical substance are macerated for ten to fourteen days in one-tenth per cent. solution of perosmic acid, and then about a week in pure glycerine. They are then crumbled up carefully, and a small, round bundle of fibres is put into a drop of glycerine, covered by thin glass, which must be supported by small wax legs, so as not to touch the specimen. A succession of light taps on the covering glass, not so as to crush the specimen, causes it to fall apart, and permits thereby the separation of the cells and fibres very readily. In this preparation are to be seen a large number of terminal medullary nerve-fibres. The medullary substance disappears, and the fibre is continued as a very fine thread, which after a short course divides still more, but then suddenly branches into a tuft of still finer fibres, which again shows the same interminable delicate transition from filaceous into granular processes as the dividing processes of the ganglion-cells.

According to this, there is a double method of termination of the medullary nerve-fibres in the cortical cerebral substance of rabbits. In one method the continuation of the axis-cylinder enters the ganglion-cells; in the other they are lost in the same fine granular fibrous substance in which the branching processes of the ganglion-cells enter.

9.—*New Observations and Experiments on the Nerves of Taste.* By Ph. Lussana. [Arch. de Physiologie Norm. et Pathologie, 1872, 23.]

First, a case is reported to prove that the sensation of taste in the anterior part of the tongue is due to the lingual nerve. The patient was a peasant-women, forty-five years old, who underwent excision of the left lingual nerve for neuralgia. About two years after a second operation, four years after the first, touch and taste were entirely gone on the corresponding side of the tongue, but a remote and disagreeable taste was felt in the back part of the tongue on the left.

Two new cases are given to prove that the sense of taste is due to the anastomosis of the chorda tympani with the lingual. In the first, recorded by Vizioli, the general sensibility was lost in the tongue and face; the gustatory sensibility remained normal. In the second, reported by Althaus, there was entire loss of sensation in the parts supplied by the fifth nerve, but taste was normal: hence it is concluded that the gustatory fibres of the lingual, are not derived from the trigeminal; if so, then, they must be derived from the facial nerve by means of the chorda tympani, for the lingual is composed of these two nerves. Three new cases are given in support of this doctrine: In one there was paralysis of the seventh nerve on the left, from a wound behind and below the ear; there was paralysis of motion, but not of sensation. Taste was entirely gone on that side. The other two were not traumatic, and motion only was implicated on one side; sensation was perfect in all parts of the face; taste, however, was gone on the paralyzed side.

Two sections are occupied, one with a reply to Vizioli in regard to Bernard's claims as advocating the same views, the other in answer to Vulpian's objections to these views.

The portion of this essay in the number for May (No. 3) is devoted to the refutation of Schiff's statement that the gustatory nerves for the anterior part of the tongue arise with the large portion of the fifth, pass through the semi-lunar ganglion, follow the second branch of the fifth, pass through the spheno-palatine ganglion, leaving it by the vidian nerve, and by this nerve enter the ganglion geniculatum of the seventh pair as well by means of the chorda as by means of the small superficial petrosal nerve, to enter the otic ganglion, and thus reach the lingual nerve of the third branch of the fifth pair. No new experiments are recorded, the results of former experiments by himself and others being used to refute Schiff's proposition.

10.—*The Signification of Fat-Granules and Granular Cells in the Cord and Brain.* By Prof. LUDWIG MEYER. [Archiv f. Psychiatrie und Nervenkrankheiten, III., 1, 2.]

The author describes the microscopic appearances of these granules and granular cells, and states that, according to his view, these formations occur exclusively in the vascular walls. When these bodies are found between the nerve-fibres, they have been carried thither by the manipulation of the preparation, but they are always most abundant around the vessels, the smaller vessels next in size to the capillaries being the ones most affected. The granules form first at either pole of the oblong nuclei which are parallel with the direction of the vessel; the granules increase until they look like a string of pearls and finally surround the whole vessel; the nuclei of the vessel change their forms, become broader, and form the oval and round granular cells, often so large as to exceed the vessel in diameter.

It is seldom that there are not other changes of the vascular walls which must be considered as consequences of the fatty degeneration, and have been long known as sclerosis, changes due to age, or obstruction of the vessels. The fat-granules may be absorbed, and in some specimens the transformation may be seen from large dark cells filled with granules into the imperfectly-defined spot on the vascular wall containing scattered fat-drops and a shrivelled nucleus. A change due to calcareous degeneration may occur, in which the vessel is completely incrusted with the chalky formation.

The multiplication of the fat-granules causes the layers of the arterial walls to be pressed out of place, the outer layers being pressed out, the inner inward, and the vessel may be so compressed as to destroy its lumen, or coagulation may occur at the roughened spot so as to obstruct the vessel.

The formation of fat granules and granular cells in the spinal cord and brain can be looked upon as forming part, probably the beginning, of a degenerative process, which under certain conditions may affect the vessels of all organs. The question may well be raised, as disturbance of innervation may be the cause of secondary changes in nutrition, whether long-continued and deeply-acting disturbance of nutrition may not react upon the innervation, depress the latter, and lead, as the next step in the process, to fatty degeneration of the vessels in the nerve-centres.

Then follows the account of forty-nine cases in which the vessels of the nerve-centres were carefully examined with the microscope immediately after the autopsy. The results are summed up, that, of forty-nine cases, omitting twenty-one which were cases of general paralysis or imperfectly observed, there remain twenty-eight cases of different diseases. In twenty-four cases, six-sevenths of all, there was fatty degeneration in both brain and cord, twice of the latter alone, and twice of the brain alone. The gray substance of the brain is the chief seat of this change. The two cases where the degeneration was not found in the brain were cases of insanity running a short course. An extensive change of the cerebral vessels was found in all the cases of epilepsy examined; the two cases where the change was wanting in the cord were cases of epilepsy. Next, diseases of the respiratory apparatus favored the fatty change in the cerebral vessels.

He considers the change to depend chiefly upon general disturbance of nutrition. When death occurred suddenly, in a well-nourished person, though an epileptic or general paralytic, the granular change in the cord was almost nothing; if the patient had been weak and emaciated, unable to leave his bed for weeks and months before death, the change in the cord was very marked.

Among the changes following this fatty granular degeneration are many changes in the vessels themselves, as ectasis, obliteration, etc., which modify the circulation of the blood and nutrition of the nerve-elements, hence passive atrophic conditions. More frequently, the pressure of the masses of fat-granules and granular cells upon the nerve-substance injures the latter, especially when the granular matter is rapidly developed.

He thinks it possible that many unpleasant sensations, as hollowness and emptiness in the head, the psychical inefficiency, sensation of weakness and pain in the muscles, such as may be met in fevers, may be due to this change.

In one case of embolism of a large cerebral artery, of at most only three days' duration, the small vessels and capillaries had already received a considerable coating of granular cells.

SURGERY.

1.—*Paget on Strangulated Hernia.* [British Medical Journal.]

The following is a synopsis of a recent lecture by Sir James Paget on strangulated hernia and its treatment, which was prefaced by the remark that, though he had operated a hundred times, to obtain conclusions of real value would need the tabulation of at least a thousand cases:

Generally speaking, in a case of hernia with signs of strangulation present, and reduction by ordinary means cannot be accomplished, an operation should at once be performed: in some cases, although the hernia is irreducible, the symptoms of strangulation are slight, obscure, or incomplete. It is an easy rule for all these cases that you should operate when strangulation is suspected: this rule you must avoid, and learn the hard one to discriminate the cases that require operation.

The irreducibility of the hernia is a fallacious sign of strangulation, and the presence of the other local signs even in a marked degree is not decisive of strangulation, and is not sufficient to prove the need of operating when the remoter signs are not present. The local characters usually present in a strangulated hernia, and sometimes the remoter signs, may be imitated in an inflamed hernia which is not strangulated. Generally, in the inflamed hernia, without strangulation, the local signs precede and greatly predominate over the remoter and general signs; while, in a hernia which is inflamed after becoming strangulated, the remoter and general signs will still predominate over the local, and the history will tell that they preceded. If these means of discrimination fail, you must operate if you cannot easily reduce the hernia; the risk of operating is small in comparison with that of waiting, for an inflamed and irreducible hernia may at any time become strangulated.

A hernia that has come down quickly, and the more it exceeds its usual size, the less is the probability of its being reduced without operation.

Again, the harder, more tense, and painful a hernia is, the less the chance of reduction without an operation.

Again, if the remote and general signs of hernia are present, and the hernia cannot be reduced, you must operate; or, if there be a swelling which may be a hernia, though it seem not likely to be a strangulated hernia, the operation must be performed at the seat of swelling.

If a patient have two herniæ that are irreducible, and signs of strangulation, and you cannot tell which is strangulated, you must operate on both.

One or more actions of the bowels, after symptoms of strangulation have set in, are of no weight against the propriety of operating; even frequent and regular action is not an absolute prohibition, as strangulation may involve only omentum or only a part of the circumference of a portion of the intestine.

As a rule, while the bowels act you should not operate, unless all the other signs of strangulation are well marked.

The sign we should most rely on as commanding the operation is vomiting. The rule is safe that recent irreducibility and vomiting are enough to justify the operation, even though there be no other signs of strangulation present. While there are notable kinds of vomiting characteristic of strangulated hernia, we should not be misguided by waiting for any particular kind. Any kind of vomiting, if it be repeated, is enough to justify operation in a hernia recently become irreducible.

Cessation of vomiting in the extreme condition of strangulated hernia is a token of evil rather than of good, if general improvement do not coincide with it. The pulse is 80 or 90 in a majority of ordinary cases in the early stages, and becomes more rapid as the symptoms of strangulation become more marked; the respirations usually are in due proportion to the pulse.

For the reduction of strangulated hernia without operation, Sir James Paget laid down the following general rules:

In cases, for instance, when the patient vomits fecal matter, and has peritonitis, or is in collapse, with a small, rapid pulse, hiccough, or other such extreme signs, there should be no attempt at reduction without operation.

When the coverings of the hernia are so inflamed as to make it probable that sloughing or suppuration has taken place beneath them, reduction should not be attempted without operation; and even when less inflamed, none but slight and brief efforts at reduction should be made.

The longer the signs of strangulation have existed the shorter should be the efforts at reduction, but the intensity of pain in recent or acute hernia should not deter one from making the attempt.

In hernia which has been habitually reducible and become strangulated, you should operate at once. It is a safe rule of practice that, after a warm bath, and a few hours' rest in bed, a single attempt at reduction should be made; should this fail, chloroform or ether should be given, and then in some cases, but not in all, a second attempt made; this failing, the operation should be performed while the patient is still insensible.

The hot bath is useful in all cases that are not bad, unless in old and feeble persons; the patient should be simply soothed or relaxed in the bath, then wrapped in warm blankets, put into bed, lying on his side or his back, with his knees drawn up, or with his pelvis a little raised, and then, after an hour or two of complete rest, to attempt the reduction. The employment of rest and the bath are helped by opium when the hernia is painful. In the old, and others who may have had inactive bowels long before the strangulation, an enema of a large quantity of liquid should be used. Purgatives should not be used if there are marked symptoms of strangulation.

After the warm bath and rest have been tried, you may give chloroform or some other anæsthetic. In making the attempt at reduction you must be gentle and self-restraining, mindful of the delicacy of some of the structures you are handling, and that you may do them much more harm than would come of the operation which you are trying to arrest. These cautions are the more necessary because, when the patient is under chloroform, you have nothing but your own sense and senses to tell you how far you may go without doing harm. Chloroform is most useful in the herniæ of which the difficulty of reduction is chiefly due to muscular resistance, in the recent, or in the recently much enlarged; in the inguinal more than in the femoral; and in these more than in the umbilical; in the painful more than in the painless. In herniæ that have only recently come down, and are intensely painful, it is right to use chloroform or ether without waiting for the influence of the warm bath, but more commonly, if there be danger in waiting three or four hours, it is because strangulation is so far advanced that the operation ought to be done without any previous attempts at reduction.

After the warm bath, rest, and chloroform have been tried, and the reduction is not accomplished and strangulation exists, you should operate while the patient is still under the influence of chloroform; but if strangulation is not present you may wait, but must watch impatiently, for the hernia is likely soon to become strangulated. While waiting, ice or warm dressings, enemata, aperients, or opiates, may be used. Tobacco and curious postures, and shaking the legs up and the head down, and the cupping-glasses, are more dangerous than the operation which they are intended to avert. For doubtful or partial reduction there is one practical rule—operate if the symptoms of strangulation are not relieved. In cases in which reduction seems complete but the symptoms of strangulation are still present, operate, if you can feel a lump at or near the hernial ring.

Old age and disease may add to the risk of an operation for strangulated hernia, but they must be accepted. A patient must not be allowed to die with a strangulated hernia, if by any means whatever the strangulation can be relieved, and you must not be averted from the operation by the number of deaths that follow it. The deaths after the operation may be fifty per cent., but the deaths due to the operation are not more than two or three per cent.

2.—*Cystic Disease of the Testes in an Infant Five and a Half Months old.* By F. H. Gross. [Philadelphia Medical Times, June 15, 1872.]

The following case was reported at a meeting of the Pathological Society of Philadelphia, held April 25, 1872:

The child was brought to the dispensary of St. Mary's Hospital about a month ago. On examination, it was found that the left testicle was enlarged to nearly the size of a pullet's-egg; hard, yet elastic to the touch, and would bear considerable pressure without giving pain. The mother said that when the child was about six weeks of age she first noticed that one testicle was slightly larger than the other, and considerably harder, describing it as "feeling like a marble," and that it continued steadily to grow until it reached the size above mentioned. The lymphatic glands of the groin were not involved, nor was there any history of syphilis or tubercle. On introducing the exploring-needle, a small drop of clear liquid oozed from the puncture. Not feeling entirely satisfied as to the character of the disease, Dr. Keen was requested to examine the case, and was very decided in pronouncing it cystic sarcoma.

Five days ago the child was etherized, and, with the assistance of Dr. Keen, and the residents at the hospital, I removed the testicle in the usual way; but, to control hæmorrhage from the spermatic artery, acupressure was employed above the surgical wound, by passing a pin underneath the cord, and looping a thin wire moderately tight over its two ends. When the cord was divided, there was no bleeding whatever, nor did it retract beyond the pin. Water-dressing was applied to the wound. On returning to my office in the evening, over six hours after the operation, I found that a message had been left about two hours previously from Dr. Nelson, the resident at the hospital, who had charge of the case, desiring me to come immediately, as the child was bleeding. On my arrival at the hospital, I found that hæmorrhage had commenced about four hours after the operation, but had ceased without serious result to the patient. The loss of blood, however, was considerable for so young a subject, having soaked through the diaper and clothing of the child on to the dress of the mother, who was holding the child in her lap before the bleeding was discovered. To guard against a recurrence, an additional loop was more tightly applied over the ends of the pin. The hæmorrhage was unexpected, but, had it been promptly detected, it could have been easily controlled in this way.

The specimen which has been divided through the middle, is seen to contain innumerable cysts, varying in size from that of a pin's head to that of a large pea, or larger, most of them filled with a clear, jelly-like fluid, with here and there one containing a brownish substance.

I intended to call special attention to several hard, bony points that were felt when the specimen was first cut through, but, after several days' maceration, I do not now feel them.

3.—*Laminaria in the Treatment of Urethral Strictures.* By Robert Newman, M. D. [Medical Record, July 1, 1872.]

The following are some of the conclusions arrived at by Dr. Newman:

1. The bougies must be made from an unblemished piece of the plant, taken out of the middle, made with care and equal in its whole length and size. If there is the slightest suspicion of unevenness, it should not be used.

2. That part of the bougie which will occupy the portion of the urethra below the last stricture, and particularly *that* part of the bougie which enters the bladder, must be varnished previously. Mastic-varnish can be used. If the varnish is applied just before using, the laminaria may dilate a little, but, if several coats are applied and allowed to dry, no expansion can take place.

3. As any oily substance hinders the expansion, no oil must be used. The bougie before introduction must be placed in cold water, until it gets a soft, velvet-like touch.

4. The bladder must be emptied before the operation, to avoid uneasiness and overdistention.

5. The urethra ought to be injected with water, to relax the parts and favor the moisture for rapid dilatation.

6. The measure of the urethra and seat of strictures must be carefully taken, notes made, and the bougie prepared accordingly.

7. The bougie, when ready, must be introduced at once, straight, without hesitation, twisting, or resting in its passage; otherwise it will cause pain, or, as dilatation goes on immediately, it will not reach the desired depth.

8. After insertion, the bougie must be left alone, and not meddled with, or tried to move.

9. It must be left inside undisturbed for from two to four hours, according to circumstances, consulting the feeling of the patient.

10. The patient during this time is left in a recumbent position, and attended or observed by the surgeon.

11. In removing the bougie the surgeon takes hold of the bougie, and uses, firmly and gradually, tractions in the same direction.

If some surgeons have failed with laminaria, they either have not observed these precautions, or they have had imperfect bougies, or selected impracticable cases.

This treatment is most indicated when the stricture is very small, almost impermeable, and no time can be lost, as the No. 1 bougie of laminaria can be introduced easier than the usual sounds or catheters. In a few hours the patient is relieved, and can micturate without difficulty. No bad results can follow, nor will it interfere with his attention to business. This latter advantage is a great consideration, as the treatment with divulsors or dilators almost always causes pain, sufferings and detention in bed and from business. As soon as the stricture is dilated so far that a steel sound of a larger calibre can be introduced, the laminaria has done its duty, and it is better to abandon its further use, and continue with other means. These are either steel sounds or galvanism.

4.—*The Alcoholic Treatment of Wounds and Atonic Sores.* By THOMAS COOKE. [Practitioner, May, 1872.]

Having read, in a recent number of the *Practitioner* (January, 1872), an interesting account by Dr. Newman of the antiseptic method of treating wounds as observed by him in a visit to Mr. Lister's *clinique*, I beg to lay before my professional brethren—rather prematurely, I fear, but for the advantage of comparison and contrast—the method of treating wounds which I learned, when in Paris, from Drs. Nélaton, Marc Sée, and Dolbeau, and which, slightly modified by myself, I almost invariably make use of in the out-patients' department of the Westminster Hospital. To state at once the position I take, I will first transcribe, with merely a few *italicized alterations* (claiming the same for my dressing), the series of advantages which Dr. Newman claims for Mr. Lister's mode of treatment: 1. The dressing is clean, inodorous, and singularly painless, *except on the first application.* 2. The formation of pus as a consequence of the injury, surgical or accidental, is, with due care, prevented *to a very great extent.* 3. Erysipelas and pyæmia, if not absolutely extinguished, are very rarely seen. 4. The wounds are free from irritation; no swelling of incised tegument and no local redness are to be noticed. 5. There is usually no constitutional disturbance (traumatic fever) after even severe operations. 6. The wounds heal rapidly.

From the foregoing lines it might be presumed that I desire to place my mode of treatment on a level with that of Prof. Lister. This is not exactly my intention. I have carefully observed the effects of Prof. Lister's treatment in the wards of Mr. John Cowper, of the London Hospital, and have there seen results more favorable than those I have yet obtained by my dressing: very large wounds and deep accidental incisions (one of which had opened the internal jugular vein) healed absolutely without suppuration, or at least without the oozing of any liquid that the naked eye could recognize as pus. I have had under my care large flaps of the scalp which have adhered in two or three days to the denuded calvarium, and suppurated only slightly along their disunited margin. I have had cases where the walls of an hematic abscess situated beneath the tendon of the occipito-frontalis muscle have adhered in three or four days. I daily see suppuration most rapidly diminish, and cicatrization, almost without any further suppuration, ensue in consequence of alcoholic dressing in what was the day before a large suppurating and inactive sore. There is, however, a certain difference between the two results. I own, therefore, that I consider my dressing to be slightly inferior to that of Prof. Lister.

But let us compare the expense of the two procedures, both with regard to instruments required, materials used, and surgeon's or dresser's time that must be sacrificed. An abscess is to be opened and dressed according to Mr. Lister's plan; I quote *en abrégé:*

1. The integument must be thoroughly washed with carbolic lotion. 2. A constant cloud of carbolic spray must be kept up over the part, and it must envelop also the operator's hand. 3. The knife employed must be dipped in carbolic oil. 4. If any vessel should have been divided, it must be tied with prepared carbolized catgut. 5. A piece of "protective" oiled-silk, coated with copal-varnish, etc., cut a little larger than the wound, must be applied after having been dipped in the lotion. On this a pad of antiseptic gauze not less than eight layers in thickness must be placed. Between the seventh and eighth layers, a layer of mackintosh cloth must be inserted. 6. A roller of antiseptic gauze must be applied—I might continue and quote further (7, 8, 9, 10) about the "*guard*" that must be used should any thing interrupt the operation; the *tent of lint* dipped in carbolized oil ought to be introduced before the sutures are inserted; the *special apparatus* required as a spray-producer during the larger operation, e. g., amputation; the very careful *filtration of the lotion* for the spray-producer; the *sponges*, which must be dipped first in a carbolic lotion of one to forty, then in a lotion of one to a hundred, etc., etc.

Now, let us suppose the same operation to be performed after the method I beg leave to recommend:

1. The abscess is opened in the usual way; any clean knife will do for the purpose. 2. The wound is to be washed first with tepid water, then with the spirit-lotion. 3. A pad of lint or some tow dipped in the lotion is to be applied to the wound, and, if the cavity of the abscess be large, partly introduced into the same. 4. A piece of gutta-percha tissue or oilcloth is to be placed over the lint or tow, and a common bandage is to be applied. The "spirit-lotion" used by Dr. Nélaton is "alcool camphré," which is nearly similar to the spiritus camphoræ of the British Pharmacopœia; Dr. Sée uses pure undiluted alcohol; Dr. Dolbeau, the same diluted with half water. My spirit-lotion is composed of equal parts of methylated spirits and common water.

The carbolic system is, as a rule, never applied in any thing like the careful manner prescribed by its author. Were it so applied, or rather, were it possible that it should be, no dressing, I believe, could supersede it. But where is the busy general practitioner, where is the surgeon to the out-patient department of almost any public charity, who could afford time to attend to one-half of the minute details above mentioned? Now, Prof. Lister's plan has utterly failed wherever any one of these has been neglected. Hence its almost complete abandonment.

The spirit-dressing is easy of application; the lotion I use is inexpensive; its therapeutical effects are very similar and almost equal to the best results ever obtained by the carbolic system. May I beg to recommend my dressing?

Miscellaneous and Scientific Notes.

Hæmorrhage from Gunshot-Wounds.—(From Prof. Billroth's "Chirurgische Briefe," of 1870. Translated by Charles E. Hackley, M. D.) The secondary hæmorrhages following gunshot-wounds come either from an opening of the vessel which was caused by the injury, and then remained closed for a time, or from an opening formed during suppuration. The former is the more frequent. The missile strikes an artery and carries away part of its wall; the shreds of tissue on the inner surface of the wound are so felted together (as in *écrasement*) that the blood cannot escape. Injury of arteries or veins does not at once cause an extensive coagulation of blood. If the shreds of tissue in the wound, which close the wound, and are subsequently fixed in their relative positions by fibrinous deposits, do not become gangrenous and are not thrown off by suppuration, the opening in the vessel remains closed. The only question is, whether the cicatrix is firm enough to prevent the subsequent formation of an aneurism, in case the artery should again become permeable at this point. This rapid closure of the wound by first intention is very rare, but undoubtedly does occur.

If the wound be torn open by movements of the patient or any other cause, at a time when the opening in the vessel is closed by adhesion of the shreds of tissue, hæmorrhage must result. The same thing happens when the fibrinous deposits soften from suppuration. Under such circumstances, bleeding could only fail to occur if the vessel had been contracted to a solid cord by its circular muscle, and had remained closed for a time, as may occur in small arteries; or if the vessel has been closed at the point of injury by a coagulum: if the vessel was only wounded on one side, the roughness there might soon cause a coagulum, either on that side or filling the vessel. Whether such a coagulum would permanently prevent hæmorrhage, would depend on its hardness and its subsequent fate; which, again, rests on the intensity of the suppuration about the vessel, as well as on the vital energy of the tissue affected.

In the great majority of cases the thrombus is merely a provisional formation, which sooner or later disappears by molecular disintegration or reabsorption of the vessels which form in it and rapidly enlarge. If the thrombus remain firm and adherent till the wound over the artery has closed, hæmorrhage does not occur, while it cannot fail in the opposite case.

The other cause of secondary hæmorrhage is the formation of an opening in the wall of the vessel during suppuration; this may occur in various ways: 1. The wall of the vessel is crushed without being torn by the projectile, and partly mortifies; the eschar is detached by suppuration, leaving an opening. 2. A splinter of bone or the sharp corner of a missile lies close to the artery, which is rubbed against it by its pulsations and is finally ulcerated. 3. From some unknown cause the suppuration assumes an ulcerative character, and the inflamed wall of the vessel softens and breaks down. In any of these cases the hæmorrhage may be prevented, or at least delayed, by thrombosis of the vessels.

Thus, it seems, there are various opportunities for the occurrence of secondary hæmorrhage after gunshot-wounds, which may also give rise to the subsequent traumatic aneurisms. Some of these causes may be proved by anatomical examination, while others can only be approximately determined. I can add nothing to the knowledge of the pathological anatomy of this subject, as I was unable to follow out the tedious examinations that would have been necessary to discover or prove any thing; but we may expect a great advance in this direction, from the late war, since such men as Hoffmann, Klebs, Arnold, Von Recklinghausen, and Cohnheim, carried on the *post-mortem* examinations at Basle, Carlsruhe, Heidelberg, Darmstadt, and Berlin.

The time when these secondary hæmorrhages occurred did not extend over many weeks; and, although the second, third, and fourth weeks were the most dangerous, they did sometimes occur in the first week.

Now, how can these hæmorrhages be most certainly and permanently arrested? I have nothing to add to what is advised for secondary bleeding from internal organs; most remedies for this purpose are only weak supports to aid the formation of a hard thrombus. In what follows we shall mostly limit ourselves to the extremities. Formerly styptics, local compression, compression of the trunk of the artery, were most frequently resorted to. In most cases the hæmorrhages were accompanied by fractures or in the neck, and these would be very much disfigured by such styptics as liquor ferri; nor could this remedy be well used in a gunshot-wound leading to a cavity full of splinters of bone, for, to be effectual, it should be applied on wads of charpie directly to the bleeding spot. I doubt very much if the thrombus induced by this means is ever organized. If the bleeding from a larger artery be once arrested by liquor ferri, the duration of its efficacy is doubtful.

Local compression cannot be long continued; else the circulation may be so much affected as to endanger the life of the limb.

Digital compression of the artery is certainly best where it can be applied. In the subclavian or carotid it cannot be borne very long, but in the brachial or femoral the patient stands it better. This plan requires a number of surgeons, or intelligent nurses, who will perform their parts patiently, and, in the cases where I tried it, it had no permanent result; but I must acknowledge it was only continued till hæmorrhage ceased to recur on slacking the compression; but it did recur the following day, and so on in from twenty-four to forty-eight hours. Of course, I cannot say whether the effect would have been more permanent if, after the first or second hæmorrhage, the digital compression had been kept up for several days and nights. But army practice is little calculated for determination of this question, as the necessary assistants are not attainable in army hospitals, and are scarce enough in civil hospitals.

The excellent results of this method of Vanzetti in certain cases of aneurism show that under some circumstances a solid thrombus may be formed. But whether such a thrombus would remain firm, if in contact with a suppurating wound, is a question which can only be decided by experience.

Among *operative* procedures, finding and ligating the bleeding vessel is the most natural and anatomically it is the most correct. But, in a cavity containing splinters of bone, callus, shreds of tissue or granulations, such an operation very often fails. It is usually necessary to make free incisions to get at the bleeding point, and meantime probably several ligatures must be applied, and the patient loses much blood. If, finally, the ligature or acupressure arrest the bleeding, it is a great gain, but usually the attempt is unsuccessful, the patient has lost much blood, has fainted several times, and still the artery must be ligated higher up.

Stromeyer, Beck, and others, advise greater boldness in ligation. This advice, lack of success in other procedures, as well as my experience in similar cases in civil practice, induced me to ligate the larger trunks rather early; but the result was not altogether satisfactory.

Formerly want of practice in the operation of ligation, and fear of gangrene, were said to be the reasons for avoiding this operation. But now neither of these objections would hold good; operating on the cadaver is so common that, for the first few years after graduation, there is probably no operation that the young surgeon could perform better than that for ligation of arteries; and experience has taught that gangrene is not apt to occur after these ligations. Only one of my ligations was followed by gangrene. Still, some of my colleagues objected to ligation in the continuity of large vessels, saying—1. It did not usually arrest the hæmorrhage permanently. 2. New hæmorrhages usually occurred at the point of ligation. My experience contradicts the first of these assertions, but to a certain extent agrees with the second.

It was even asserted that ligation in the continuity for arrest of hæmorrhage was nonsense; that, in a system of communicating tubes, the escape of fluid from an opening could not be prevented by closing another tube. It is unnecessary to dispute this question here, as cases given in a previous letter show that ligation always arrested the bleeding at the original point of injury. I cannot explain how this happens: probably, after ligation of the trunk, a coagulum forms in and around the bleeding artery, which plugs the opening till it is closed by granulation, which may occur in from one to twenty days. If the thrombus lasts so long, and then there be no serious mechanical injury to the wound, the adhesion or cicatrization of the opening becomes solid enough to resist the blood-pressure.

But we must consider at greater length the second objection to ligation of large arteries in their continuity, that is, secondary hæmorrhage from the point of ligation, from the ligature coming away too soon, as it is said. Following my preceptor's teachings, as well as my impressions derived from recent literature, and from my own investigations, I have always regarded it as a wonderful provision of Dame Nature that the larger the vessel the later the ligature separated, and supposed that the thrombus thus gained more time to acquire the necessary firmness and union with the walls. But, unfortunately, the pressure of the ligature often causes mortification of the coats of the vessel so early that hæmorrhage results, or, what is just as bad, even after a long time (thirty to fifty-five days), the thrombus may be so soft when the ligature separates as to permit hæmorrhage.

But what most astonished me, and something that I would not have believed without having repeatedly seen it, is, that, in an autopsy even several days after ligation, there may be no thrombus, or it may only be on one side; also, that, in large vessels especially, thrombi are often short and incompletely developed.

To make statistics as to the frequency of hæmorrhage from different vessels or different points, it seems to me only proper to use those cases where the cure went on regularly, and those where hæmorrhage occurred at the point of ligation; those where the patient died, either from pyæmia or bleeding, before the ligature came away, should not be counted at all, for we cannot say whether secondary hæmorrhage from the point of ligation would have occurred or not if death had not interfered. It seems to me that in this respect Porta has made a mistake in his master-work, "Delle alteraziona patologiche delle arterie per la ligatura e la torsione."

We must investigate more closely the anatomical conditions after ligations, to satisfy ourselves about what is at present obscure, and derive profit for the future. Since the existence of a thrombus has been known, its action in arresting hæmorrhage has been disputed. While Petit and Morand thought it was the chief factor in arresting bleeding, Ponteau, Jones, John Bell, and Stilling, supposed that adhesion of the soft parts above the vessel, and finally adhesion of the walls of the vessel, were the causes. The importance of the thrombus was again claimed by Zwicky, Virchow, Weber, Rindfleisch, Bubnoff, and Kocher, while it has recently been denied by Tschausoff and Roser.

Porta attributes an equal effect to the thrombus, and to condensation of the enveloping tissue, the "inviluppo limfatico," in the permanent arrest of hæmorrhage. From my own investigations I found that in the thrombus, in place of the coagulated blood, vascular, firm connective tissue, rich in cells, made its appearance, and expressed the opinion that even this organized thrombus after ligation was only provisional; it disappears, and subsequently the artery becomes conical, pervious to its end, and is then transformed to connective tissue.

The excellent statistical works in the "Archiv für klinische Chirurgie" by Pilz (ligation of the carotid), Koch (ligation of subclavian), Kocher (ligation of femoral and iliac), G. Fischer (aneurisms of gluteal, etc.), have brought together an immense amount of material to show the frequency of secondary hæmorrhage after the ligation of large arteries.

The certain effect of acupressure, continued for only forty-eight hours, even in the femoral artery, hardly agrees with the thrombus doctrine which has been for a time generally accepted; for, if arrest of hæmorrhage were always due to thrombus, and if, even in the femoral artery, this were firm enough at the end of two days to permit no new bleeding, how comes it that the thrombus in a femoral, three weeks after ligation in the continuity, is not firm enough to prevent bleeding after the ligature has cut through? When this accident happens after three or four weeks, it is very remarkable, for it is hardly probable that there could be such a difference, as between two days and six weeks in consolidation of the thrombus.

It seems to me that the result of examinations of patients who have died with the ligatures still in position, is very important in settling the question whether the thrombus is the sole cause of arrest of hæmorrhage. Referring to those cases that have come under my own observation, in a case of ligation of both carotids, fourteen days after ligation of the right carotid (the ligature having separated without hæmorrhage on the day of death), the artery was completely closed for over two inches by a firm thrombus, closely adherent to the wall of the vessel; this reached downward, and up into the external carotid, but was there less consistent than below. On the left side where the common carotid was ligated forty-eight hours before death, the artery when opened showed no thrombus either above or below the ligature, neither was the intima cut through, but the wall of the vessel was so folded together that it was doubtless impermeable. In the surgical museum at Zürich, there was a carotid with a thrombus after ligation, which was hardly three lines long.

Of the cases mentioned in my previous letters, I may call your attention to the following: In Case XX., "in the iliac artery, a small thrombus (death in twenty-four hours);" in Case XXIII., "at the point of ligation (of the external iliac, forty-eight hours before death), no thrombus either above or below;" in Case XXIV., where death occurred in seven days by hæmorrhage from the point of ligation of external iliac, it is stated that the upper and lower thrombi only measured three to four lines, were of moderate consistence, and not particularly adherent to the wall of the vessel. If you glance at the tables of statistics above mentioned, you will see that, not unfrequently, the thrombi were very short and spongy, or were entirely absent.

Porta has given us numerous observations as to the frequency of thrombus after ligation of the large arteries of animals. In two hundred and fifty cases, where the arteries were examined several days after ligation, he found no thrombi in thirty-five cases. As Porta attaches great importance to the thrombus, he seems to suppose that secondary hæmorrhage would have occurred in all these cases; but there were also fourteen per cent. of secondary hæmorrhages in these cases, which would nearly correspond to the twelve and a half of secondary hæmorrhages which he previously estimated as occurring in man.

The above observations have led me to believe that *healing of a wounded artery may occur without formation of a thrombus.* All of us have probably sometimes seen, on autopsy, that the thrombus does not always reach to the next branch of the artery, as it is expected to do, and often does. But we cannot fail to be astonished at finding no thrombus; under these circumstances, we could only maintain the view that the thrombus alone effected the permanent arrest of hæmorrhage, by supposing that, in all cases where it was absent, too short or too soft, hæmorrhage must occur from the point of ligation. This conclusion is not absolutely certain—it only rests on a certain majority of observations.

Now, what is the cause of the inequality in the formation of thrombus? This is not only a difficult question, but it is entirely unanswerable till we know more than we now do about the coagulation of the blood. The general view is, that most coagulations in the vascular system are due to retardation or absolute rest of the current of blood—that they are directly caused by foreign bodies in the vessels. Popular as this view has become from Virchow's writings, and fluently as surgeons talk of "dilatation, compression, marasmatic, etc., thrombi, emboli, etc.," they have really added little to the explanation of the phenomena of the coagulation of the blood.

In his classical work on this subject, Brücke shows that, in living vessels, quiet of the blood alone will not induce coagulation; so, of course, retardation of the circulation would be just as little likely to have this effect. In the venous sinuses, in some portions of varicose veins, and in many cavernous vascular tumors, the blood moves very little; and, although small coagula do occur here and there without special cause, still extensive coagulation only results from some irritation, mostly inflammatory in its nature. Brücke ascribes to the living walls of the vessel the power of maintaining the fluidity of the blood; he says (Virchow's Archiv, Bd. xii., p. 100): "I think I have now shown clearly that the blood in cold-blooded animals is kept fluid by the action of the walls of the heart and blood-vessels, and on escaping coagulates, because no longer influenced by this cause." Subsequently he shows that, in general, this is also true of warm-blooded animals, and does not depend on a negative quality of the vessels, but that they actively oppose coagulation of the blood (page 183). "I cannot say wherein this power lies." In the preceding interesting pages he tries to make the findings on pathological anatomy agree with his view.

Richardson's view, that blood is kept fluid by the ammonia it contains, is disputed by many, among others by Lister (*Edinburgh Medical Journal*, April, 1858, December, 1859). The interesting investigations of Alex. Schmidt, in Dorpat, are very striking and important in explaining the nature of fibrine, but I cannot see that they much advance our knowledge of thrombosis.

Sometimes, when I think about renewing my experimental studies of the thrombus, my courage fails on looking over the works of Virchow, Brücke, and Porta. So I can only try by those works to explain the observations we have made.

The supposition that quiet of the blood was enough to cause its coagulation, induced the idea that, after ligation, the thrombus always extended to the next collateral branch; but this does not always occur: perhaps it is hardly the rule, which would show that the above supposition was incorrect. According to Brücke, quiet of the blood in a detached portion of a vessel will only induce coagulation when properties of the living coats of the vessel are thereby impaired; thus, it is probably one condition of the normal life of the coats of the vessel that new blood should constantly flow over it; if this ceases, its property of keeping the blood fluid is impaired: hence quiet of the blood in a vessel would only remotely favor its coagulation; and we may imagine that, in spite of quiet of the blood in a vessel, its peculiar vitality might, under some circumstances, be preserved for a length of time, or even permanently, so that no thrombus would form, as in some of the cases previously mentioned.

In the ligation of arteries, the most important factor is the mechanical influence of the ligature on the coats of the vessel, and its exposure. Whether the ligature only be applied tightly enough to obstruct the flow of blood, or be drawn so firmly as to cut through the intima, its application always modifies or removes the vitality of coats of the vessel at this point; the disturbance will extend to the vicinity, and though we may imagine this part, where the function is unimpaired, as being very small, a thrombus should always form there; I cannot state positively whether it was always absent in the cases where we failed to see it, or whether it would have only been discoverable by the lens or microscope. Persons in whom I found no thrombus a day or two after ligation were, it is true, affected with anæmia, septicæmia, or pyæmia, at the time of the operation, but it is difficult to see how this should have influenced the action of the coats of the vessels. It is said that in anæmic persons the blood coagulates very readily, and the same thing has been ascribed to pyæmia. Why, then, do we have no thrombus? I cannot explain it; it seems as if we still sought blindfolded after these processes. In regard to the relation of veins and capillaries to inflammation, the discovery of wandering cells passing out from them, and into the thrombus through the vessel, seems to show that there is some coincident softening of the walls of the vessels, and this pathological condition may impair the power the vessels have of keeping the blood fluid. But, unfortunately, we cannot apply this to the formation of thrombi in arteries; for up to the present time the passage of wandering cells through the walls of arteries has not been observed.

All of which shows how little we really know on this subject. But still a practical result, even if a negative one, may be deduced, viz., *that we cannot certainly influence the formation of a thrombus, and still less can we determine its length, either by a choice of the point of ligation, or by any other circumstance relating to the operation.*

The practical importance of the rupture of the intima of the artery, which almost always occurs to some extent on the formation of thrombus, was first and most correctly recognized by Jones. Kocher also refers to it in his excellent work on acupressure. Still the smallness and occasional absence of thrombi after acupressure and ligation show that the effect of this cause of coagulation is not very extensive, and that the thrombi, discoverable only with the lens or the microscope, furnish no absolute security against hæmorrhage after a ligature has cut through quickly. Jones and Kocher, in advancing the injury to the intima, as the cause of thrombus, evidently think it is caused by the mechanical influence of the roughness caused by the ligation; while Brücke appears to ascribe it more to a chemical influence, speaking of the action of the living vessel on the blood.

When Kocher states, as the result of his careful investigations, that closure of the artery is always due to thrombus, not to direct adhesion of the inner surfaces, it may be correct histologically; but it cannot mean that, after ligation and acupressure, the hæmorrhage is checked *only* by the thrombus. On the contrary, his published observations say, "In acupressure, at the time the needle is removed, the arrest of the hæmorrhage may be entirely due to the walls of the vessel."

Let us study more closely *the closure of wounds in arteries without the formation of thrombi.* I find that I formerly paid too little attention to this point, and accepted the thrombus doctrine so firmly that it required my recent observations in the army hospitals, and repeated readings of Kocher's work, and the recent criticisms of Roser, to induce me to investigate this process again.

I have frequently employed acupressure, but have rarely cut open the large arteries where it had been resorted to, when making autopsies of those who had died after amputation. I supposed *a priori* that a thrombus formed there, as it usually does after ligation. If this is not always the case, and still hæmorrhage does not recur, the arrest of bleeding can only result from the compressed or twisted end of the artery, remaining fixed in the position where it has been brought by the needle, and which has prevented the flow of blood. Kocher has shown that this fixation is not caused by a firm adhesion of the intima at the points where it is in close contact. It is unnecessary to say that the blood-clot, of which there is often only a trace, could not restrain the current of such an artery as the femoral, two days after the injury. Moreover, acutorsion and acupressure do not cause felting of the arterial tissue, or rolling in of the coats, as *écrasement* would. Small arteries might close by muscular contraction, and remain closed till some other means rendered the closure permanent; large arteries have little muscular tissue, hence cannot contract so as to close entirely. So the only thing left is the divided artery, which has been closed by acupressure or torsion, remains closed, and is fixed in position by the fibrine (coagulated lymph, or exudation) which exists or is deposited in the tissues of the wounded surface. This very coherent, firm, and contractile substance is gradually transformed into granulation, and then into cicatricial tissue; it retains the artery in position; at first it adheres, and finally grows fast to it; thus the opening in the artery remains closed. Any one having had occasion to open a stump two or three days after an amputation, on account of retention of pus, if it has mostly healed by first intention, will know how firm these adhesions are, and will not find it strange that this adhesive material suffices to keep the artery closed after the removal of the needles. It is somewhat more difficult to understand the closure of a ligated artery which contains no thrombus. The elastic coats of the artery must fold up under the ligature. If the portion compressed by the ligature mortifies, or is softened by ulceration (if the ligature was not tied tight enough to instantly destroy the vitality of the vessel), we generally suppose that the vessel has been closed by a thrombus, that has become firm and adherent to the walls of the vessel; I think I may conclude that the thrombus usually disappears subsequently, and that finally the artery is closed only by the cicatrix, from the fact that, in none of the healed amputation-stumps that I examined, did I find any remains of thrombi, and the arteries always had conical ends. Should secondary hæmorrhage occur in all cases where there is no thrombus, when the ring-shaped eschar caused by the ligature comes away? This is difficult to answer; but I think that even under these circumstances the conical end of the artery would be held by the fibrine on the surface of the wound; the opening left by the falling of the ligature might contract, granulations would grow over the end of the vessel, and close it permanently, provided the ligature did not cut through too quickly. Kocher very aptly compares this process with the closure of a small opening in the intestine by Cooper's method, where "peritonæum and mucous membrane are seized together by the forceps, and a ligature applied. Here mucous membrane is pressed against mucous membrane, but adhesion does not take place till they are cut through, and the edges of the peritonæum come together."

Let me add one other point: after detachment of the ring-shaped eschar, the elastic artery doubtless has a tendency to reopen, and recover its cylindrical form; this may be prevented by the surrounding fibrine and granulations, as above shown. But the end of the artery, which has always been exposed to some extent, has also produced granulations on the outer surface, which, from the conical shape of the end of the artery behind the ligature, and its tendency to concentric contraction, offer considerable opposition to the separation of the folds of the artery. Lastly, the inflammation about the ligature may impair the elasticity of the intima.

Hence we may imagine the closure of a ligated artery without the formation of a thrombus. At the same time we find that, if a thrombus does not form, acutorsion is surer than the ligature.

[TO BE CONCLUDED IN OUR NEXT ISSUE.]

Medico-Legal Remarks concerning Hair. By Dr. E. HOFMANN. (*Vierteljahrschrift f. d. Praktische Heilk.*, 4, 1871.)—The examination of the hair in its medico-legal relations is a subject hitherto but little noticed, except superficially in the "Yearbook of Legal Medicine." Yet many cases might be mentioned in which the microscopic examination of the hair was of great importance.

In the medico-legal examination of hair, two questions are met:

1. Are the hairs from animals or from men?
2. In the latter case from whom do they come? From what portion of the body?

Of course, if the hairs belong to a beast, that may be sufficient to settle the question at issue; but the difference between such and human hair has been too little noticed. A human hair under the microscope shows three distinct layers: the outer, cuticula, or the superficial covering, formed of epithelial cells, with rounded contour, lying over each other like tiles, which clothes the surface of the hair from its exit from the skin to its end. The ends of the scale stand out somewhat from the shaft, and give the outer circumference of the hair a more or less jagged appearance. Seen sideways, the cuticula appears as an undulatory design, more prominent if the hair is treated for a short time with concentrated acid. The scales have their points directed toward the free end of the hair, hence the latter can be easily distinguished from the other broken end.

The cortical substance forms the principal part, and often the whole of the shaft. It consists of a system of closely-packed cells in rows, lying nearly parallel to the long axis of the hair, giving the cortical substance an appearance as if striped lengthwise. These cells are so intimately united that without reagents this striped appearance alone shows the cellular structure. Concentrated sulphuric acid breaks up this union, and reveals the spindle-shaped cells, with occasionally a nucleus.

The cortical substance has different color, according to the color of the hair; generally the color is diffused through its whole mass; less frequently the color depends on granular pigment scattered through its substance in small masses.

Finally, the cortical substance contains a number of cavities filled with air, most evident in the hair from aged persons or in dry hair. These are secondary results of drying, as they are not found in young hair.

The central portion, the medullary substance, forms, when well developed, an axis-cylinder, one-fifth or one-fourth the diameter of the hair, with sharp outlines, generally central, but many times a little eccentric in position. The medullary substance is not constant; it is often wanting in human hair, especially in blond hair. It is wanting less frequently in hair obtained from other parts of the body than in that from the head. In woolly hair it is always wanting; also in the hair of the new-born child. The medullary substance is often interrupted, and sometimes consists only of a few dark points lying in the axis of the hair.

The nature of the medullary substance is still a matter of dispute, some considering it cellular, others denying this. The first is certainly the correct view, as may be seen by following the development of the medullary substance from the papilla, where round and imperfectly-polygonal cells can be seen gradually merging into the medullary substance.

The medullary substance has been thought to contain the pigment; this is not so, the supposed pigment-granules being very minute air-bubbles. The cause of the color of the hair is found in the diffuse pigmentation of the cortical substance. The cause for the hair becoming gray or white is to be found in the disappearance of the diffuse pigmentation of the cortical substance, the cause of which is not yet known. The medullary substance can be more easily seen in white hair than in colored.

Turning now to the hair of animals, we find generally the same three layers as in human hair, but differing to such a degree that, as a rule, a hair can be easily recognized as belonging to an animal. The cuticula in most animals has absolutely and relatively larger cells, which give the hair a characteristic appearance, as is seen especially well in the wool from sheep. A toothed or saw-like appearance of the contour of certain animal hairs depends upon the larger development and peculiar relations of the cuticular cells, whose points stand out so far from the hair that the latter has a feathered appearance, as in the field-mouse. Among animals the greater bulk of the hair is formed by the medullary substance, the cortical substance being only a thin layer; often, indeed, is reduced to a hem-like streak. This predominance of the medullary substance is seen best in the shaft of the hair; toward the end the cortical substance predominates, the medullary becoming thinner. Generally, the cortical substance has the same structure as in human hair, and the same variety of pigmentation; in some animals, as the cat, rat, and mouse, the cortical substance is more translucent and of finer structure, resembling, under the microscope, a hyaline envelope of the medullary substance.

The medullary substance in animals is an interesting study, differing greatly from the same layer in human hair. The cellular structure is generally very evident, without the employment of any reagent. The cells vary greatly in size and form.

(Then follows a description of several varieties of hair from animals.)

Though the hair of animals usually is so different from human hair that it can be easily recognized, yet the difference is sometimes less marked; especially may this be the case with single hairs, and at times only a single hair can be had for examination. This resemblance is caused by the absence of the medullary substance. Dogs' hair, especially when brown, is often very similar to human hair, or may be almost exactly the same; fortunately, only separate hairs are thus similar, while generally the remaining hairs which are given for examination have clearly the animal type. Reagents will often help to decide the question.

In medico-legal cases, when it has been decided that the hair examined is human hair, the question arises, from whom it comes and from what portion of the body. In regard to the first question it may be merely said here that the hair examined must be compared with that of the person concerned, both in regard to its gross appearances and microscopically.

In deciding to what part of the body the hairs belong, the length, the size, the form, and the root of the hair, must be noticed.

The hair from the head and beard is less limited in its length than the hair on other portions of the body; though individual and other circumstances may modify the length of the hair from the head and the beard.

The size of the hair differs in different parts of the body, and so may form a diagnostic mark. The beard is the thickest generally, measuring 0.14 to 0.15 mm. Next comes the hair about the female genitals, 0.15 mm.; then the eyebrows, 0.12 mm.; the hair about the male genitals, 0.11 mm.; finally, the hair from the head in either sex, 0.06 to 0.08 mm. The great individual differences which are found may render the value of the size for diagnosis less valuable. Moreover, it must not be forgotten that the same hair may vary in diameter. The shape of the hair modifies its diameter; thus cylindrical hair especially is found only on the head; but when this is curly it is flattened, and the transverse section is then oval instead of round. The beard is generally triangular on transverse section, with one convex side; the hair from the genitals is generally oval, sometimes triangular. Hair which has been exposed to the action of the sweat is sometimes swollen in one part, and so changed in form.

When the hair grows undisturbed it ends always in a fine point. All the hair of a new-born child, hair which grows at the age of puberty, and such as has grown naturally without interference, always has a pointed end, which may be of use in deciding in regard to the age of a person. Later this normal ending is not found. Hair which has been cut has at first a sharply-defined transverse section; later the edges are rounded off, and the end becomes round and diminished in size, or is frayed out. This may lead to an approximate calculation of the time which has elapsed since the hair was last cut. The beard, being less frequently cut, is more often split and frayed out. The hair from the female head, generally not cut, ends regularly in two to three points, often in more, each having the end frayed out.

The shape taken by the ends of the hair depends upon the action of friction and sweat, the former splitting and rubbing off the ends, the latter macerating and acting chemically by dissolving or softening the connective substance. The shaft of the hair is acted upon by the same agents and changed; especially active is the sweat, changing the color, as is seen in the axilla, on the scrotum, and the labia.

From the form of the hair, especially of its end, we can draw conclusions as to the nature of the influence to which it has been exposed, and by means of this and its other peculiarities we may be able in medico-legal cases, with more or less certainty, to decide from what part of the body it came. But no form of hair is absolutely characteristic of any portion of the body.—S. G. WEBBER.

Appointments, Honors, etc.—The degree of Doctor of Civil Law has recently been conferred by the University of Oxford, England, on Samuel D. Gross, M. D., Professor of Surgery in the Jefferson Medical College, Philadelphia. Prof. William T. Lusk, M. D., has had conferred upon him by the Faculty of Yale College the honorary degree of Master of Arts. Prof.

Wm. Warren Greene, of the Bowdoin Medical College, Maine, has accepted the chair of Principles and Practice of Surgery and Clinical Surgery in the Long Island College Hospital, Brooklyn, New York, in place of Prof. Crosby, resigned. Prof. T. G. Richardson has been transferred from the chair of Anatomy in the University of Louisiana to that of Surgery in the same institution, made vacant by the retirement of Prof. Stone; and Prof. Samnel Logan, formerly of the New Orleans School of Medicine, has been appointed Professor of Anatomy. Dr. S. A. Green having declined the election as Superintendent of the Boston City Hospital, Dr. Edward Cowles, recently of the U. S. Army, has been appointed to that position. It is announced that Dr. William Stokes, Regius Professor of Physic in Trinity College, Dublin, and Physician to the Queen in Ireland, is about to receive a baronetcy. Dr. Stokes is the acknowledged head of the profession in Ireland. Dr. Julius Nicolaysen, of Christiania, Norway, has been appointed to the Professorship of Surgery in the University of Christiania. Profs. Billroth and Brücke, of Vienna, have been offered chairs in the University of Strasbourg. They have, however, decided on remaining in Vienna. Dr. Arthur Gamgee, Lecturer on Physiology in the Edinburgh Medical School, has been appointed Examiner in Forensic Medicine in the University of London. The Waynflete Professorship of Chemistry at the University of Oxford has been conferred on Prof. William Odling, some time Fullerian Professor of that subject at the Royal Institution. Dr. Bence Jones, in consequence of ill-health, has resigned his seat in the Council of the Royal College of Physicians. Dr. Meadows has been elected a corresponding member of the Imperial Medical Society of Vilna. Dr. Paget, Regius Professor of Physic, and Dr. Humphry, Professor of Anatomy, have been elected to represent the University of Cambridge on the English Examining Board. Dr. Hughes Bennett, Professor of the Institutes of Medicine in the University of Edinburgh, has returned from Italy and the south of France so far reinstated in health as to commence his clinical course in the Royal Infirmary. Dr. Augustus P. Simpson, late Professor of Medical Jurisprudence in the Andersonian University, has been appointed to the chair of the same department of Medicine in the University of Glasgow, which had become vacant by the resignation of Prof. Rainey.

Diphtheritic Paralysis.—Dr. Mühsam (*All. Med. Cent. Zeitung*, 4, 1872) believes that there are three points of difference between the paralysis which follows diphtheria and that which follows cholera, typhus, and small-pox. In the latter, paralysis follows only severe cases, while after diphtheria it is found after very light cases where the local disturbance is limited; secondly, in diphtheritic paralysis a long time often elapses between the primary disease and the occurrence of the paralysis, while in the other diseases the succession is more close; and thirdly, after diphtheria, a definite group of muscles are affected typically, while after other diseases the most different groups of muscles may be affected. He considers that the cause of the paralysis is to be found in an affection of the sympathetic excited by the diphtheritic poison. In support of this view, he reports two cases in which the sympathetic was evidently disturbed in its action. In one the heart was functionally affected, in the other the facial muscles and the vaso-motor nerves. He considers this implication of the sympathetic as one cause of the danger attending the disease.

Prevention of Pitting in Variola.—Dr. A. L. Leach (*Philadelphia Medical Times*) claims to have obtained excellent results from the use, in confluent variola, of the ordinary carbonate of lead, brought to the consistency of cream by the addition of linseed-oil, and applied to the face with a camel's-hair brush; a few applications form a complete covering, excluding access of air and light, and allaying irritation. Dr. Leach mentions incidentally that in the delirium of small-pox he has found signal benefit from the use of bromide of calcium.

Ligature of the Common Carotid.—Ligature of the common carotid, even in our London hospitals, may be regarded of such rare occurrence as to deserve special notice when the operation happens to be performed. Mr. Wagstaffe tied the artery on Monday night at St. Thomas's in a man who suddenly lost a large quantity of arterial blood five days after his upper jaw had been removed for a tumor affecting it. Mr. Wagstaffe used the catgut ligature, and cut the ends short, leaving them in the wound. The man on Wednesday was doing well, and the wound was healing by first intention.—*Lancet*, June 22d.

Therapeutic Use of Creosote.—An anonymous donor has placed at the disposal of the medical faculty of the University of Leipsic the sum of three hundred thalers (about $225) as a prize for the best essay on "The Therapeutic Uses of chemically pure Wood-Creosote." The donor expresses the hope that some useful result will be arrived at as to the application of creosote to malignant tumors. The essays are to be sent in to the Leipsic medical faculty up to March 31, 1873; and their relative merit will be tested by clinical experiment.

The Methodists and Quackery.—The action of the last General Methodist Conference in regard to excluding quack advertisements from all church papers is worthy of hearty approval, and is a step in the right direction. It is to be hoped that the committee to whom the question was referred will make a report on the side of morality and decency, and thus deal a heavy blow at the charlatans who have been accustomed to find religious journals only too ready to aid them in deluding a credulous public.

Cold Food for Infants.—Dr. J. H. T. King, U. S. A., in a communication to the *Philadelphia Medical Times*, strongly recommends the use of *cold* milk in rearing infants on artificial food. He believes the tendency to gastric and intestinal disorders is much less when the feeding-bottle is kept in cold or ice water than when the milk is raised to the temperature of mother's milk. He has found also that infants relish cold food, and that its effect is particularly good during the teething period.

Educated Garroters.—We learn from the *Medical Times and Gazette* that recently Dr. F. Kirkpatrick, Vice-President of the Royal College of Surgeons, Ireland, while proceeding to visit a patient at ten o'clock at night, was garroted in one of the most fashionable streets of Dublin, and deprived of his watch and chain. One of a gang of three men quickly rendered Dr. Kirkpatrick insensible by pressing firmly on the carotid arteries on both sides.

The Topeka Medical College.—"College of Physicians and Surgeons" has an imposing sound, and hence is a favorite title with young colleges. The Topeka College of Physicians and Surgeons claims to have a fine building, hospital, dispensary, etc.; but the *Leavenworth Medical Herald* unpatriotically warns students who thirst for medical knowledge to seek it elsewhere.

The New York Medical Register.—The present volume of this useful work, for 1872–'73, is by far the most complete and comprehensive that has yet been issued. In addition to very full information regarding the physicians of this city and State, it contains a record of those of New Jersey and Connecticut, together with much other matter of value and interest to the profession.

An Apology.—We inadvertently neglected to give credit to Dr. Henry D. Nicholl, of this city, for the admirable *résumé* of Cohnheim's views on Embolism, published in the July number of this JOURNAL. We were indebted to the same gentleman for a translation and summary of Simon's recent publications on the Surgery of the Kidneys, etc., given in our April number.

International Ophthalmological Congress.—The Executive Committee of the International Ophthalmological Congress in 1872 announces that it will be held on August 1st, 2d, and 3d, at the Royal College of Physicians, Pall Mall, London.

The Detroit Medical College.—The regular lecture term of this college has hitherto been held in the spring and summer months. Hereafter the course of instruction will begin on the first Wednesday in October, and continue five months. There will be a preliminary term, which will open on the first Wednesday in September.

Chairs for Invalids.—A great variety of easy-chairs, with every possible mechanical device for adding to the comfort and convenience of invalids, are manufactured by H. Thompson, 70 East Broadway, who has for many years given special attention to this class of furniture, and its adaptation to the requirements of the profession.

The Medical Schools of France.—M. Jules Simon has intimated his intention of reorganizing the medical curriculum in the various schools of France. He proposes to maintain the faculties of medicine as they now exist, to extend that of Paris, and to create new faculties at Bordeaux, Lyons, Nantes, Lille, and Nancy.

The New Board of Pharmacy.—The College of Pharmacy of the City of New York, in accordance with the new law, has elected Dr. Wm. Manlius Smith, Dr. Wm. Neergaard, Dr. F. H. Weismann, Paul Balluff, and Theodore Frohwein, as a Board of Pharmacy, to serve for the next three years.

A New Medical College.—A new medical school, under the title of the College of Physicians and Surgeons, has been organized in Wilmington, North Corolina. Dr. J. Francis King is President, and Dr. Wm. Walter Lane Dean.

Minutes of the American Medical Association.—We have received a copy of the minutes of the last meeting of this Association, which have been carefully arranged and edited by the permanent Secretary, Dr. Wm. B. Atkinson.

Effects of the Excessive Heat.—The deaths in this city during the week ending July 8th were 1,569. In Boston, during the same week, there were 228 deaths, and in Philadelphia 764.

Small-pox in Boston.—There were reported in Boston, during the three months ending June 30th, 373 cases of small-pox, of which 101 proved fatal.

Foreign Correspondence. The Use of Anæsthetics abroad.—The following communication has been sent us from Vienna, by P. A. O'Connell, M. D., of Boston:

While the surgeons of Boston, and the medical profession generally, of Massachusetts, are reposing a quiet confidence in the safeness and efficiency of sulphuric ether, as a means of preventing pain in surgical operations, anæsthesia in Vienna seems to be either in its infancy or in its "dotage." In the hospital, the surgeons of which, I presume, may be considered fairly as reflecting the most advanced views of the profession here on this subject, one is struck with the hesitation and evident fear with which anæsthesia is attempted. The vacillation, the indecision, with which different means of preventing pain in operations are tried, show with how little confidence the various agents are regarded. And the lack of agreement among the surgeons, as to the means and methods of producing the effect desired, in a safe and justifiable manner, indicates how fully they feel that, on the subject of anæsthesia, they are "at sea," without a compass to guide.

My attention was attracted to the existence of this state of things, first, in a pointed manner, upon entering the operating-room of Prof. Billroth. I had witnessed the administration of chloroform—or rather the pretence of administering it here—several times, in the obstetric department, particularly in an operation for vesico-vaginal fistula, by Prof. Braun, in which the fear of the anæsthetic was so much stronger than the desire to relieve the patient of pain, that she was obliged to undergo the operation with very little assistance from anæsthesia. But, reasoning that Prof. Braun was an obstetrician rather than a surgeon, I found an excuse for his hesitation in the free use of chloroform, and did not consider the circumstance as specially worthy of note. The timidity with which the subject was treated in Prof. Billroth's operating-room, however, the extraordinary caution used, and the very great length of time consumed in bringing patients under the influence of the anæsthetic, surprised me. I had not spoken with anybody upon the subject before entering the operating-room, and consequently was not prepared for the exhibition that presented; but it, itself, prompted me to make inquiry; and my surprise became less when I learned that the professor had, within a couple of years, lost two patients, under the influence of chloroform; and that, since then, very properly, he has been afraid of it. Now he uses, in this very timid manner, a combination of chloroform, ether, and alcohol. During the operation, however, the fear of the anæsthetic continued to manifest itself very decidedly; much more time was consumed in bringing the patient under the influence of this mixture than it would have taken to etherize several patients in succession; and this influence was kept up so hesitatingly after the operation was begun, that the patient felt, although not very sensitively, perhaps, what was going on, and expressed his dissatisfaction by his halloos and struggles.

I do not know whether Prof. Di—l has ever met with an accident in the use of chloroform as an anæsthetic; so far as I have been able to learn, he has not; but, if not, his confidence in this agent seems to have become shaken by the experience of others; and he would, no doubt, gladly avoid the use of it, if he could be persuaded that something else was, even nearly, as efficient and more safe. The same want of confidence in the article—the same groping, in search of a substitute for it, that manifests itself in Prof. Billroth's operations, and in his mixture of it with alcohol and ether, is exhibited by Prof. Di—l, too; and in an operation for lithotomy, by this gentleman, that I had an opportunity of witnessing, the chaotic condition of anæsthesia here was exhibited very well. First, an attempt was made to guard against the dangers of chloroform, by mixing it with two parts of ether; using, for the purposes of inhalation, a bag made by uniting two large pig-bladders, to which had been fastened a mouth-piece consisting of a scoop-shaped appendix of leather, capable of covering both mouth and nostrils. This bag had a small button-hole in the side, for the admission of air, and, after pouring in nearly a tablespoonful of the mixture, the mouth-piece was applied over the patient's face so as to oblige him to breathe through the apparatus. Thus it was retained for ten minutes; but the effect of the application was not very satisfactory; for, at the end of that time, the patient was as wide awake, almost, as if no attempt at anæsthesia had been made; and eventually, as if in despair, chloroform was resorted to. By the cautious use of this agent, in eleven minutes more, the patient was pronounced ready for the operation. Among the objections urged against the use of sulphuric ether, the length of time consumed in placing the patient under its influence is one upon which the advocates of the use of chloroform generally lay a particular stress. Although this objection ceases to have weight, under any circumstances, when the safety of the patient is taken into consideration, it will be seen that, in the practice of the gentlemen here, it can have no weight whatever; because, in the use of *their* anæsthetics, they consume more time to produce the effect than is necessary in the use of ether. When considering time, however, in connection with the use of ether, it is worthy of note that, with a person unaccustomed to the use of this agent, much time will be wasted unnecessarily in the attempt to place a patient under its influence. A physician or surgeon who has been accustomed to the use of chloroform, for instance, will naturally use ether at first in the same cautious manner with which he used the chloroform; and the time he wastes, while learning to use ether properly, is credited, of course, to the inefficiency of the article. I have met with a gentleman here, a physician of intelligence—an American, too—who stated that, after trying *an hour and a half*, he had found it impossible to produce anæsthesia with sulphuric ether; and, attributing his failure to the agent itself, of course, rather than to his own ignorance of how to use it, he had come to the conclusion that, so far as anæsthesia was concerned, ether was a failure. This gentleman had been appointed lecturer on materia medica to an American medical college, and of course was in a position to influence the opinions of others in relation to the value of drugs and medicines. He wished to have nothing further to do with ether as an anæsthetic.

But to return to Prof. Di—l: he succeeded at last in getting his patient anæsthetized, and proceeded with the operation. The man lay upon his back, with his legs drawn up on to the abdomen, in the usual position for lithotomy; two assistants were stationed at each leg to keep them in position; another had charge of the anæsthetic; another held the sound; and numbers of others were on hand to make themselves useful, or get in the way, as opportunity offered.

Every thing being in readiness, the incisions were begun—at the same time the influence of the chloroform began too—i. e., *to pass off;* and the same timidity, as to its further application, which characterized the use of the anæsthetic in Prof. Billroth's operations, was displayed here; so that it was with difficulty that the man could be kept in proper position; the assistants who had charge of the legs brought all their strength into action to overcome the disposition to kick, manifested by the patient; and their heated, red faces, from which huge drops of perspiration rolled freely, testified that their failure did not result from want of exertion. The patient struggled, the assistants struggled, and the surgeon swore, i. e., mentally; giving vent to his feelings in an angry *sac-r-r-r-r*, which he checked, however, as he remembered himself, and rounded off into a bitter statement that, unless the man was kept in position, he could not operate; and, for a short time, the scene was decidedly exciting. But the stone was extracted at last; quietness resumed its sway; everybody who had taken part in the operation seemed fully satisfied; and the wild struggle that had just occurred excited no comment whatever, everybody seeming to look upon it as the regular and proper manner in which such things should be done.

Among the other surgeons of the hospital, the same sense of insecurity in the use of chloroform exists, and the same yearning for a safe substitute is manifested—louder, perhaps, in action, than they would be willing to acknowledge in words; for, in spite of misfortune in the use of this agent, by Prof. Dum—r, his assistant enrolls him still as one of the "unterrified;" and Dr. Sa—r resorts to chloroform when *compelled* to use any anæsthetic. But anæsthesia is not resorted to upon trivial occasions; man's endurance is tested fully; and the shadow of death that is reflected from the chloroform-bottle, to some extent, to everybody who uses this agent, seems to assume a particularly threatening aspect to the surgeons in Vienna.

When taking into consideration the fear of chloroform that is manifested so openly here, and knowing that this fear of it is based upon good grounds—upon some sad experiences of the results that may follow its use—I have wondered how it happened that, during my military experience, when I saw chloroform used by surgeons of all kinds, skilled and unskilled, in a very free manner, in some instances as freely, almost, as ether is misused, deaths did not occur under its influence. During the war of secession, chloroform was our stand-by in the field. Operations of all kinds were performed with it as the anæsthetic; and with heaps of mangled men, awaiting their turn to be attended to, but little time was given to precautions against its dangers. Indeed, as I look back upon that time, and recall with how much of what now, in civil life, would be termed recklessness, the article was administered, I wonder at the results. Surgeons were using it freely, every day, using it with impunity, accidents did not occur to remind them of the possibility of their happening; and the fact recorded in civil life, that chloroform was capable of producing death, became to them a myth. Nobody was unfortunate in the use of chloroform, nobody expected to be; and, as special caution in the administration of it, certainly, was not the cause of the "lack of mortality," from this source, how can this be accounted for rationally?

My own experience with chloroform would justify me in using it very freely, because I have found it to operate pleasantly, efficiently, and harmlessly; but as it is a fact beyond dispute or question that deaths *may* occur from its use without any warning that we can recognize, and in a manner against which, as yet, we know not how to guard; and, as this possibility of death exists in every case; it is assuming an unnecessary risk, for no possible proportionate gain, when a surgeon who has become familiar with the use of ether makes use of chloroform instead, as an anæsthetic.

Thus far I have referred, in plain terms, to the surgical practice in Vienna, and endeavored to exhibit fairly the condition of anæsthesia as it exists here; but while it is really a matter of congratulation that somebody has stumbled into the use of a safe anæsthetic, the surgeons of Boston have but little reason to take unto themseves much credit for the present standing of sulphuric ether. It is an *inheritance*, for the *possession* of which alone the present generation certainly is not entitled to much commendation. Since the discovery and introduction of this agent, little or nothing has been done to advance its usefulness, or to remove the objections which exist against its use; and, indeed, the indifference with which it is regarded outside of Massachusetts, and the carelessness with which it is used even there, the lack of interest that prevails as to the diffusion of the knowledge of its proper use and value, indicate rather that its present foothold is purely accidental, and favor the belief that, if the anæsthetic properties of ether had been made known *after* the discovery of those of chloroform intead of *before*, no school of medicine would have been found so rational as to become its champion. Outside of Boston, ether is used comparatively little, and this to a great extent because the proper way to use it is not known.

In illustration of this, I may refer back to the case, already mentioned, of the physician who, after a prolonged and honest effort, failed to produce anæsthesia with it, and the disagreeable after-consequences of its use. The vomiting-sickness, etc., that are apt to follow, as well as the unpleasant preliminary of its administration, very unpleasant when compared with that of chloroform, prevent persons educated in the use of this latter agent from becoming willing to consider the claims of ether without prejudice, even when looking for something that they may use with safety. In reference to this latter point—the very innocence of the article—the impunity, so far as life is concerned, with which it may be used in a reckless manner, has been allowed to militate against it, even in its *home*; and at the public administrations of this agent, where students are supposed to learn how the article should be used, and what its natural effects are, it is not unusual to see sponges applied so over-saturated with ether as to be incapable of retaining their contents; and, what is worse, to see them applied over the mouth and nose of a patient lying on his back, with his face pointing directly upward, so that the drainage must unavoidably *flow* into his pharynx, choking him, adding unnecessarily to the unpleasantness of the administration, and rendering subsequent vomiting and sickness a matter of course. No doubt this manner of administration convinces students of the perfect safety of ether; but, unfortunately, it does not, at the same time add to its reputation; and, when the common laws of gravity are disregarded so glaringly where this agent is concerned, it is not surprising that nothing positive has been accomplished toward removing the objections that do actually and fairly exist against it.

Judging from what I have seen, it seems to me that the great impediment to the more general use of ether is, ignorance of how to use it. When chloroform becomes too terrifying, recourse is had rather to *mixtures* than to ether, because, in reference to this latter, strong prejudices against it exist among those who know little about it. Familiarity with it would spread its use even now; and if, in addition, it were possible to discover a means of preventing the disagreeable effects of the article with certainty, a new and a wide field of usefulness would open to it, particularly among such gentlemen as these of Vienna, who are in darkness, and are seeking for light.

The fear of chloroform, and the desire for a safe substitute for it, are not limited to Vienna. The British Isles, even, look upon it as a necessary *evil;* and since writing the foregoing, in Vienna, I have, here in Berlin, seen in the London *Lancet* of April 27th, also in the *British Medical Journal* of May 25th, serious recommendations from surgeons of experience, that a galvanic battery, ready for immediate use, be the accompaniment of the chloroform-bottle, as is stated, first, "because the use of the anæsthetic is never safe;" and, secondly, "because the galvanic battery is the most speedy and certain means of resuscitation." A colleague, who agrees (*British Medical Journal*, June 1st) in the truth and importance of the statement, suggests, however, that the use of the galvanic battery is not without danger; for a powerful electric shock, sent through the par vagum, will paralyze the heart, and stop its action. They had better end their difficulties by adopting the use of sulphuric ether.

The Late Dr. E. B. Dalton.—The following resolutions and remarks on the death of Dr. E. B. Dalton were offered by Dr. George A. Peters, at a meeting of the Medical and Surgical Society of New York, held June 8, 1872.

GEORGE G. WHEELOCK, M. D., *Secretary.*

Whereas, it hath pleased the All-wise to remove from among men our associate, Dr. E. B. Dalton:

Resolved, That this Society has received with profound regret the news of the death of Dr. Dalton.

Resolved, That in his death a bright light in our profession has gone out, and a void is left which it will be hard to fill.

Resolved, That we cherish his memory as that of one who, by a life of self-sacrifice and devotion to duty, shed lustre upon a noble profession, and left a record which all who come after may study to advantage.

Resolbed, That a copy of these resolutions be published in the medical journals.

Resolved, That a copy be forwarded to his brothers and sister, as an expression of the sympathy we feel for them in their affliction.

At the last meeting of this Society, held at the house of Dr. McLane, I was requested to offer, at the next meeting, resolutions expressing our sense of the loss which we had sustained in the death of Dr. E. B. Dalton.

I cannot present these resolutions without saying a word or two in testimony of the rare character and great worth of their subject.

I have known Dr. Dalton ever since the commencement of his professional career, and during a part of that time we were intimately associated. I can truly say that no friendship, formed after the heat and fervor of early youth had passed, has so entwined itself about my heart.

You will all bear witness to the truth of what I say, when I claim that Dalton was one of those rare characters whom we seldom meet.

Guileless as a child, he was as fearless and true as any knight of the olden time.

With great sweetness of temper he combined great firmness and determination. His executive power was such that he seemed born to command.

All these qualities were called into play during the war of the rebellion. Actuated by motives of the purest patriotism, he enlisted heart and soul in the cause. Commencing as an assistant-surgeon in one of our early volunteer regiments, he rose rapidly from rank to rank until during the last, long, memorable campaign, in charge of all the sick and wounded at the base, he filled a position second to none in importance and responsibility.

Never physically strong, the exposure and hardships to which he was subjected planted in his constitution the seeds of that disease which finally terminated his life.

Many of you will doubtless well remember his worn and weary look, when he was brought North by kind friends who went to seek him, and found him perishing in the pestilent swamps of the Chickahominy.

He remained with us only long enough to recover partially, and then, urged by that sense of duty which always impelled him, he returned to the field of his duty and labor, never again to leave it until, "the victory won," he could throw aside the trappings of war, and mingle with us here in the city of his adoption.

The remarkable faculty for command, and "for evolving order out of chaos," which he had shown in the army, indicated him as the man eminently fitted to organize the Board of Health, which was to be established under the new charter in this city.

He was made Sanitary Superintendent. Most of the measures which were adopted for improving the health of the city, and preventing the spread of contagion, were due to his foresight and fearless performance of duty.

When the commission came to be a political machine he left it in disgust, to devote himself to private practice, in which he was admirably fitted to achieve marked distinction.

Now came upon him that terrible avalanche of affliction, which swept from him wife, family, and what remained to him of physical health. His mind and courage it could not crush. He was, under it all, the same uncomplaining, sympathetic, gentle nature. A trip to Europe benefited him somewhat, but after an attempt to settle in Boston, and practise his profession in the home of his brothers and early friends, he was obliged to relinquish the effort and seek for health in some more genial clime.

Last autumn he left us for the Pacific slope, hoping that in the open-air life of that salubrious region he might find the healing spring.

A few weeks ago we heard of his death. Just how he died I have not yet learned, but that he was surrounded by brothers and kind friends I doubt not.

He has gone to join those whom he loved so dearly. He is now beyond the veil, through which for these years past he has been gazing so intently. He has left none braver and truer behind him.

May we all so live that, when we come to die, we may leave behind us a memory as green and fragrant as that of Edward Barry Dalton.

Singular Congenital Malformation.—Assistant-Surgeon J. F. P. McConnell reports in the *Indian Medical Gazette* of January 1, 1872, the following remarkable case:

Woochoop C. S., a Bengali lad, aged twenty-five, was born in the village of Connihatty, Zillah Rajshahye, and presents the following peculiar malformations:

(*a.*) *Spine.*—An angulo-lateral curvature is seen occupying the whole of the dorsal region; the lateral deflexion is toward the right; all the ribs from the third downward participate in the curve, and are greatly bent at their angles.

(*b.*) *Hands* (1) *Right.*—Fingers normal, but the *metacarpal bone* of the thumb is bifid, i. e., consists of two "rami" articulating by a common base to the trapezium, but separating at right angles from each other almost immediately: the summit of *each ramus* is surmounted by two phalanges, which are moderately well developed, and each *distal one* bears a well-formed nail. (2) *Left.*—Fingers also normal. The metacarpal bone of the thumb is *single*, but supports *four phalanges*, two of which are situated vertically with respect to the head of this bone; the other two, rudimentary in character, project laterally outward from this point, as in *abnormal developments of the sesamoid bone* usually found in this situation.

(*c.*) *Ears.—Almost Complete Absence.*—On the *right* side of the head is observed (in place of the external ear) merely a small reduplication of integument analogous to the superior fold of the "pinna," and a rudimentary "tragus." The orifice of the auditory canal is *impervious*, being closed by integument continuous with that of the side of the face, but in this situation it is very delicate, thin, and free from fat. On the *left* side of the head, a rudimentary "lobule" and "antitragus" alone exist, the external orifice of the auditory canal being closed in the same way as on the opposite side.

REMARKS.—There is no malformation or peculiarity about the feet or any other part of the body, nor is there any history of the hereditary transmission of these deformities. The lad is remarkably intelligent, can both read and write the native character: the latter accomplishment he seems very proud of, and performs quickly and with apparent ease, holding the pen between the forefinger and double-thumb of the right hand. He is *deaf*, but not completely so by any means: if, in speaking to him, the mouth is held close to the auditory canal, he can hear very fairly; as a rule, however, he prefers being spoken to *through his mouth*, i. e., in conversing he invariably keeps his mouth wide *open*, and then, if the voice be raised to a moderately loud pitch, he can hear remarkably well. This would seem to prove that the "Eustachian tubes" are pervious, and form the channel for conduction of sound.

In conclusion, I may add, that instead of considering himself an object of commiseration, he is, on the contrary, rather proud of his singular peculiarities, and makes a good deal of money by "exhibiting" himself. I trust this brief account of his condition will not prove uninteresting to those who seek to investigate and record the many wonderful *lusus naturæ.*

Legislation against Quackery.—The following is the text of the recent law relative to the practice of medicine in Eastern and Central Pennsylvania:

SECTION 2. It shall be unlawful for any person to commence the practice of medicine or surgery in the counties of Dauphin, Chester, Carbon, Luzerne, Mercer, Erie, Blair, Bradford, Sullivan, Crawford, Beaver, Monroe, Washington, Venango, Lycoming, Huntingdon, Schuylkill, Lawrence, Somerset, Philadelphia, York, Union, and Adams, who has not graduated with the degree of doctor of medicine, and received a diploma from a chartered medical school or other institution authorized to grant medical diplomas: *Provided*, That the provisions of this section shall not apply to persons who have been ten years in continuous regular practice in said counties, though they may not have graduated as aforesaid, nor to persons who are reading medicine under the control and instruction of a physician or surgeon who has the qualification to practise prescribed by this section, when such person shall have the assent of such preceptor to practise: *Provided, however*, That such student shall not locate any office or business station outside the usual office of his preceptor.

SEC. 3. Any person who shall attempt to practise medicine or surgery, by opening a transient office in the aforesaid counties, or who shall, by hand-bill or other form of written or printed advertisement, assign such transient office, or other place, to meet persons seeking medical or surgical advice or prescription, shall, before being allowed to practise as aforesaid, appear before the clerk of the courts of the county, and shall furnish satisfactory evidence to such clerk of the courts that the provisions of Section 1 of this act have been complied with, and shall, in addition, take out a license for one year, by payment of a license fee, for the use of the county, of two hundred dollars: *Provided*, That the provisions of this act shall not apply to dentists: *And provided further*, That physicians or surgeons commencing practice in any of the aforesaid counties, with the intention of remaining permanently therein, shall not be subject to the provisions of Section 1 of this act.

SEC. 4. If any person shall violate any of the provisions of this act, every such person shall be guilty of a misdemeanor, and on conviction thereof shall be fined in a sum not exceeding five hundred dollars, or less than two hundred dollars, or imprisoned not exceeding six months, at the discretion of the court.

Aspiration in Strangulated Hernia.—At the meeting of the *Académie de Médecine* on May 21st, M. Demarquay presented a man, twenty-one years of age, in whom he had reduced a strangulated congenital inguinal hernia by the aid of aspiration. On May 5th a tumor appeared in the left groin, accompanied by severe pains and vomiting, which persisted next day. At the end of twenty-four hours he was taken to the Paris *Maison de Santé*, where the taxis was employed without success. Ice was applied during the next twelve hours, when M. Demarquay saw the patient. His features had undergone great change, and fever was set up. A congenital, elongated, voluminous inguinal hernia was found to exist, and M. Demarquay paid the more attention to other measures, inasmuch as he had never succeeded in curing this description of hernia by operation. He applied carefully the taxis, while the patient was put into a deep sleep, with no effect, and he determined to try the effect of removing the intestinal liquids and gases by means of aspiration. A fine trocar was passed into the centre of the tumor, and, by means of Potain's aspirator, about one hundred and twenty grammes of intestinal liquid were drawn into the recipient. The tumor subsided completely, and, the trocar having been removed, some minutes were allowed to elapse without touching the tumor, in order to observe whether new liquids or gases would enter the strangulated intestine. No renewal of the tumefaction took place, and very slight pressure upward sufficed to procure the return of the intestine into the cavity of the abdomen. The patient was kept quiet, and on low diet, fractional doses of opium being administered. No ill consequence followed. The case M. Demarquay regards as striking, and he proposes to apply this new mode of treatment—1. In all congenital hernias, and to recent hernias which become strangulated at the time of their formation. 2. To old hernias which were quite reducible a few days prior to strangulation, and in large umbilical hernias that have been recently strangulated. 3. Aspiration, which has for its object facilitating the employment of the taxis, should only be employed at an early period, when one can be wellnigh certain of returning into the abdomen the intestine in an unaltered state, and capable of resuming its functions.—*Medical Times and Gazette.*

Glycerined Lymph.—The *Medical Times and Gazette*, in an article on the preservation of vaccine lymph in glycerine, says that six years have now elapsed since the discovery of its advantages, and Dr. Müller, during this period, has been untiringly endeavoring to make these known more widely. The late war, occurring amid the prevalence of small-pox, has amply demonstrated its value; for it was found that, on the outbreak of the disease, the only provision in Germany of lymph capable of meeting the emergency existed at the Berlin Institution, and supplies had to be obtained from it for revaccination in Baden, and for revaccinating the Saxon and Würtemburg armies. Without the aid of glycerine the immense supplies for both civil and military practitioners, at home and abroad, never could have been sent; and the army surgeons can best judge what influence the withholding of such revaccination might have had on the course of the campaign. Still, in general only enough lymph could be sent to set the revaccination process going, the revaccinating from arm to arm still continuing to be practised. In fact, but few military surgeons prepare the glycerined lymph themselves, although this can be so easily done. If during the summer each were to vaccinate a few healthy children, he might lay by a stock of glycerined lymph (which keeps for months or years) that would enable him to revaccinate whole batches of recruits on the same day.

Dr. Müller employs equal parts of chemically pure glycerine and distilled water. These are thoroughly mixed in a watch-glass by means of a fine pencil. The proportion of one part of lymph to five of this diluted glycerine forms a good inoculable matter. If immediate vaccination is desired, the lancet should be charged by means of the pencil. If it be desired to keep the lymph, it may be put either into capillary tubes or in a phial holding half or a whole drachm. This need not be filled, and has only to be secured by a cork; but before using the lymph it ought to be rubbed up again with a pencil. The lymph in this way may be kept for months, especially when placed between glass plates. It should not be exposed to the light, but warmth does not injure it.

A Humorous Journal.—The *Leavenworth Medical Herald*, which, by-the-way, has undergone some improvement of late, manifests a certain lack of respect for a well known medical writer, concerning whom it indulges in the following remarks, headed "Mental Conceptions:" One Dr. Z. C. McElroy, of Ohio, has for the past two years inflicted upon the various medical journals of the West a variety of lucubrations of a decidedly transcendental order. He certainly is a funny fellow, and we acknowledge ourselves indebted to him for many a hearty laugh; "but yet the pity of it, Iago!"

As a specimen of his style, we quote from his last mental evolution as published in the May number of the *Medical Archives:*

"My diagnosis was therefore, motion in material in the interest of repair nearly at a stand-still; while the waste of his various structures in the performance of function notably prominent in his nasal passages, cavities, and brain, was proceeding at a rate corresponding with the elevation of temperature above the natural or physiological velocity.

"My mental conceptions in investigating and prescribing for the case, etc.

"Without these definite mental conceptions, etc.

"But with my clear mental conceptions of what was actually taking place in the body of my patient, etc.

"I selected opium, and selected it because I had, or thought I had, definite mental conceptions, etc.

"And I thought these definite mental conceptions, etc.

"As it was, I think its successful termination in so short a time, and with so little medicine, was clearly due to my mental conceptions of the state of my patient," etc.

We are delighted to learn (the *Herald* continues) that the doctor has left the sea of doubt and uncertainty, and floated out on the ocean of absolute knowledge; for, while calmly reposing upon its transparent bosom, he can direct his ardent gaze toward "the brave o'erhanging firmament, that majestical roof fretted with golden fire," and indulge in *mental conceptions* of the unknowable, with an absolutely inappreciable waste of brain-structure.

Destruction of Life by an Elephant.—In our September number we chronicled the destruction of twenty-one lives in the Mandla district in about twenty days, by an elephant. In a supplement to the *Central Provinces Gazette*, dated 16th December, we observe that this same animal killed eighteen persons (fourteen males and four females) between the 17th October and 7th November, in the Balaghat district. The story of his ravages is told by Captain Bloomfield, the Deputy Commissioner of that district. The brute proceeded from village to village, upsetting *machans* (sleeping-places for watching cornfields and granaries), knocking down houses, consuming rice and grain, mangling and killing every human being he could get a hold of, and spreading consternation all over the district. He was killed by the Deputy Commissioner and District Superintendent of Police with a party of armed constables, on the 7th. He was a full-grown male, measuring 20 feet 9 inches from the tip of his trunk to the end of his tail; his height 9 feet 5 inches; breadth of skin 19 feet 2 inches and length of tusks 2 feet 5 inches. His history is thus told:

"About twenty years ago, when the estate of the Nawab of Ellishpore was escheated to the Nizam's government, this elephant escaped and made its way into the jungles of Chhindwara. The Rajah of Nagpore hearing of it, offered a reward for its capture, and sent two detachments of sowars after it. Hunted about the Chhindwara district, it descended the ghats, and passing close to the city of Nagpore turned northward and took to the hills at the north of the Halla pergunnah of this district. For several years it roamed the Dhansua hill and then went away north of Bhimlat to the Bhainsa ghat range, where it remained without doing much damage to man or property, until the beginning of the year, when it distinguished itself by killing twenty-one human beings in the Mandla district." A reward of two hundred rupees was given for its destruction.—*Indian Medical Gazette*, February 1, 1872.

Quarterly Report of the St. Franciscus Hospital, New York City.—During the quarter beginning January 1, 1872, and ending March 31, 1872, there were received into the hospital 290 patients, of which number 192 were males, and 98 females. Of the former the greater number were young, unmarried mechanics without any means; most of the female patients were servant-girls. The St. Franciscus Hospital, belonging to the Sisters of the Poor of St. Franciscus, is purely a hospital for the poor sick. The physicians connected with the hospital are: Dr. Achilles, Physician in Charge; Dr. Carl H. Lellmann, Consulting Physician for Internal Diseases; Dr. Hermann F. Guleke, Consulting and Attending Surgeon; Prof. Dr. Joseph Kammerer, Consulting and Attending Physician for Diseases of Women. Dr. Kammerer holds a clinic on Monday and Thursday from 11 to 12 A. M., for gynæcological cases. Of the 290 patients received during the quarter there were discharged cured, 134; transferred to the public hospitals as incurable, 15; died, 51; remained in the hospital on March 31, 1872, 90.—*Medical Record.*

The Best Position for the Reduction of Strangulated Hernia.—Dr. Simorre recommends (*L'Abeille Médicale*, May 27, 1872) that in the reduction of a strangulated hernia the patient should be placed in the following position: A mattress having been doubled on itself, the patient is placed upon the inclined plane which results, in such a manner that his buttocks will rest on the most elevated part, and the front of the vertebral column, which corresponds to the epigastrium, upon the lowest part. His head and chest are supported by a bolster, and should be higher than the epigastrium, so that the muscles of the front of the neck, and of the chest and abdomen, shall be in a condition of extreme relaxation. The space at the foot of the bed made vacant by the doubling of the mattress should be filled up, so that the patient's feet may have a suitable support. He says that, since he has had recourse to this method, in no instance has a strangulated intestine resisted his efforts at reduction.

Cerebro-spinal Meningitis in the East.—Dr. Sandreczky, physician to the German Hospital in Jerusalem, writing in the *Berliner Klin. Wochenschr.* of May 13th, remarks that it is generally said that cerebro-spinal meningitis does not occur in the East, or, at least, that it has not been observed there, Within the last few weeks, however, he has for the first time met with five cases in hospital and one in private practice. The patients were all young, apparently aged from ten to fifteen; three were males, and three females; two died, and two were still under treatment. Regarding the prevailing atmospheric and other influences, Dr. Sandreczky says that the winter was very rainy; that the *sirocco* or *simoom* set in earlier than usual in the spring, and was followed by alternations of heat and east and south winds with cold north and west winds. The condition of the population from which the cases came was miserable, on account of last year's drought and scanty harvest.

The March of Cholera.—A late number of the *Journal de Génève* says: The caravan which left Mecca on February 25th with an effective of four thousand pilgrims, arrived on March 10th at the tomb of the prophet at Medina. During the fortnight four hundred persons died of cholera. There were at that moment at Medina seven thousand strangers, among whom the scourge was raging with terrible severity. Before the arrival of the Damascus caravan it appeared to have abated, but it immediately broke out again with violence.

Obituary.

J. C. Weeden Cooke, M. R. C. S., died in England in the latter part of May, at the age of fifty-six. He was well known as a surgeon, and as the author of several valuable papers on surgical topics. The *Times and Gazette* says of him: "He was rather heavy than brilliant—rather 'slow' than decided. He was an amiable man, and much esteemed by those who knew him."

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ART. I.—*Yellow Fever as it has occurred in Wilmington, North Carolina, from* 1800 *to* 1872, *being an Examination of the Review of Dr. Thomas, on the Report of Dr. Wragg* (NEW YORK MEDICAL JOURNAL, December, 1869). By E. A. ANDERSON, M. D., Wilmington, N. C., December, 1871.

"Audi alteram partem."

"DIFFERENT opinions are held upon almost every subject not susceptible of positive demonstration, and opposite conclusions are drawn from the same facts and circumstances, just as they have been seen by the narrator, or related to him by others. The influence of preconceived opinions and adopted theories often gives direction to the judgment without a consciousness of the fact." To these very expressive words of my friend Dr. Thomas I give my most unqualified assent; but the results of my observations and experience in yellow fever differ far from those of the able writer whose review I approach with much diffidence and reluctance. I had hoped some more able pen would have vindicated the paper of Dr. Wragg; but, as more than a year has elapsed since the appearance of the review of Dr. Thomas, and no reply has been made to it as yet, I feel myself called upon, at this late hour, to support the opinions of one who is now justly regarded as the highest living authority on yellow fever, and who witnessed personally the epidemic he so well describes in his able paper of August, 1869. A more extended experience of yellow fever since 1862, up to the time I am writing (December, 1871), satisfies me that yellow fever can, and does, originate in the latitude of Wilmington, N. C., without importation from more southern and tropical regions.

The yellow fever of 1862 existed in Wilmington before the arrival of the steamer Kate, as was stated in the paper of Dr. Wragg. Most of these statements were furnished Dr. Wragg by the writer, then recovering from an attack of yellow fever. They were then fresh in my memory, and I had no doubt of their correctness at the time they were made. Seven years subsequently this paper was reviewed by my friend Dr. Thomas, in a candid, calm, and courteously-professional manner. Like myself, Dr. Thomas seeks only the truth, but the result of his researches differs far from mine. In his very able communication, the doctor takes the ground that no facts prove the existence of yellow fever previous to the arrival of the steamer Kate from Nassau; that it was imported by this steamer, and that it did not exist in Wilmington previous to her arrival. Dr. Thomas was taken sick in the middle of the epidemic, October 14, 1862, and obliged to leave for Louisburg, one hundred and fifty miles distant, in the northern part of the State, to recruit his health, where he had a very severe attack of yellow fever, with black-vomit. Hence most of his statements were necessarily second-hand, and all or most of his facts derived entirely from the recollections of parties seven years subsequent to the epidemic, or from the very imperfect cemetery records. These records were at that period so imperfectly kept that, desiring, a few years later, to find the time of the death of a friend, in order to erect a monument over her grave, I could discover no record of her death whatever. This was caused by the death of Mr. Quigly, the superintendent of the cemetery, by the terror, dismay, and dread of the new pestilence in our midst, and by the disorganization of war which deranged most of the wheels of civil life. The parties from whom Dr. Thomas derived his information trusted alone to their memory, and, having no notes of the cases, were more liable to mistakes than those who wrote on the spot while the facts were fresh, and but of yesterday. It has been well observed by Horace:

> "Segnius irritant animos demissa per aures
> Quam quæ sunt oculis, subjecta fidelibus."

> "Mere words upon the ear dull and unheeded lie,
> Enduring still, what seen by the all-faithful eye."

Commencing practice, as many hundred other physicians, with an utter disbelief of the local origin of yellow fever, and believing devoutly in its importation, facts that I have observed compelled me to change my views; and, although I neither hope nor wish to change those of others, still the strong desire to invite inquiry, and promote calm and dispassionate observation prompts me to bring these few statements before the notice of the profession. In the terrible epidemic of 1862 I treated over two hundred cases of yellow fever individually, and suffered from an attack myself. No one then can deny, for a moment, my power of detecting it, or impugn my motives for pronouncing upon its nature, because they claim it cannot exist except in tropical climates, or spring up save by importation. The design of this article is to show that sporadic cases have been met in an unbroken chain since 1862; and have been described by De Rosette, Hart, and Hill, as far back as 1800, 1819, and 1820. Sporadic cases of yellow fever have shown themselves in this city every year, but have either passed unnoticed by the physicians, or have not been reported from the fear of producing unnecessary alarm and panic in an excitable community that still remembers with dread the terrible epidemic of 1862. Intense heat, and morbific conditions of the atmosphere during the summer months, are amply adequate to the production of yellow fever, which has seldom appeared anywhere without being preceded by a high degree and prolongation of atmospheric heat, to say nothing of the existence of local causes of insalubrity. The conditions for the spread of yellow fever are three, namely: a certain heat, a certain moisture, and a certain effluvia from animal or vegetable matter, all of which existed in this city in 1862. Fevers are the endemics of Carolina, and occur oftener than any, probably than all, other diseases. They are the effects of its low grounds and stagnant waters. In the opinion of Dr. Ramsay, the great historian of South Carolina, in the mildest seasons they assume the type of intermittents, in the next grades they are bilious remittents, and under peculiar circumstances, in their highest grades, constitute yellow fever. Prof. Dickson remarks that the causes of yellow fever are in perpetual existence in Charleston in the summer atmosphere. Charleston, and other cities of the United States, charge Havana with inflicting this terrible disease upon them; and Havana, on the other hand, attributes the sin of yellow fever to Siam. Be this true or not, it is very certain that it existed in Greece. And the very same disease that now prevails in the West Indies, Charleston, and other cities on the Atlantic coast, was described by Hippocrates nearly three hundred years before the birth of Christ. This illustrious Greek, says Prof. Potter, observed the disease he so well described in the mild climate of his native soil, almost in the parallel of latitude in which we live. He speaks a language without disguise, susceptible of but one interpretation. The *tout ensemble* of his faithful picture portrays the disease in colors as glowing as those of Chisholm, Rush, Geddings, or Dickson. He enumerates the more prominent symptoms, under the appellations, *καυσος* (causos), a burning inflammatory fever, attended with excessive thirst; *τυρος* (typhus), a stupor or coma; *φρενιτις* (phrenitis), an inflammation of the brain, or its investing membrane, acute delirium (?); *῞Ικῖερος* (icteros), a yellowness of the skin (?); and he caps the climax of the malignant picture by the words *Μελανα εμετον* (melana emeton), black-vomit; and *Μελανων εμετον* (melanon emeton), the vomiting of black matter. In burning fevers, says Hippocrates, yellowness of the skin on the fifth day, especially if accompanied by singultus, is a sign of great malignancy. These symptoms we see in every epidemic in Charleston. In 1800 Dr. Ramsay writes: "The disputes about the origin of yellow fever, which have agitated the Northern States, have never existed in Charleston. There is but one opinion among the physicians and inhabitants, and that is, that the disease was neither imported nor contagious." My own opinion is, that yellow fever is a local disease originating in the air of Charleston. Yellow fever has been produced by local causes far north of Charleston. It broke out in the most decided manner among the convicts of the New York State-prison in 1805, in a situation entirely inaccessible to imported contagion. The disease originated from local causes in the Richmond (Virginia) Penitentiary in 1806.—(*Medical Repository*, vol. x.)

The best-established opinion is that the fever is not the least infectious; that it rather passes from the air to the patient, than from the patient to the air. The disease cannot be propagated from one place to another by contagion, unless the atmosphere is in a fit state to produce it. In November of this year, a fatal case from Charleston arrived in our midst, but died out with its unfortunate victim. The occurrence of sporadic cases of yellow fever in any place may with propriety be regarded as a valid reason in favor of the doctrine of local origin. That these have occurred constantly in various places we have the best authority to believe, and in this paper I intend to show that they have occurred ever since the great epidemic of 1862. As has been well observed by Dr. Drake, the occurrence of the fever from local conditions, independent of any foreign agency, being established in even a single instance, the controversy is *de jure* at an end. For, if it can be thus generated in one case, it may in all, and to say that it is not, is to superadd an hypothetical to a known cause. He, indeed, who admits its production in this manner in one instance, and denies it in another, is bound to support his denial—he brings on himself the *onus probandi*. When a phenomenon has been proved to arise from a certain cause, true logic requires us to refer it, whenever and wherever it may afterward appear, to the same cause. If, then, the yellow fever in Memphis, in Galliopolis, at Natchez, in the bay of St. Louis, in the Richmond prison, in the village of Brandywine, on board of Mississippi steamers and flat-boats, in Appalachicola, and in many other towns and villages, arose from local causes alone; if some of the epidemics of Philadelphia, New York, New Orleans, Mobile, etc., are shown to have been produced by similar agencies, and if there it has not been propagated by contagion, we must conclude that it has arisen in every other place, and in the same place at every other time, from like causes; in other words, that it is indigenous, and has everywhere and always spread independently of a contagious principle (La Roche on "Yellow Fever," vol. ii., p. 468).

Description and Topography of Wilmington, N. C., in 1800 *and* 1821, *where there occurred Two Epidemics of Yellow Fever, the first slight, and the second very severe in proportion to its Population, then from* 2,500. *to* 3,000 *Inhabitants.*

In Wilmington, N. C., a town situated in a low, flat, swampy country, the fever of 1820 began on the margin of the river, and prevailed there with the utmost intensity. As to the condition of this infected locality, we may form an idea from the statement of Dr. Nathaniel Hill, an eminent physician, a pupil of Cullen, and a graduate of Edinburgh, to whom we are indebted for an account of this fever (*Medical Recorder*, vol. lxxxvi.). Dr. Hill states that the parts of the town adjacent to the river are but a few feet elevated above its surface. The wharves are made ground, badly constructed, and are always overflowed by storms, and frequently by high tides. In the vicinity of the square most and earliest ravaged by the disease, there is one of those wharves in an unfinished state, partly filled up with decaying vegetable matter which, enclosed by logs, and successively acted upon by the tides, exhibits a most loathsome and putrefactive source of disease. Our docks are notoriously filthy, and our cellars are so low and damp as in wet seasons to require daily bailing (just as in 1862, forty years subsequently). Dr. Hart, of Wilmington, N. C., a contemporary of Hill, and a very eminent writer and physician, states that in the town of Wilmington there is no intercourse with the yellow-fever region from the end of May until November, and very little at any time. Yet at the period the rice-fields are drained, cases of yellow fever occur among the unacclimated, and he has seen entire crews of vessels from New York, or some other Northern port, arriving at that season, cut off in seven or eight days. The epidemic of 1821 was clearly shown by Dr. Hill to have arisen from local causes (*Medical Recorder*, vol. lxxxvii.). The venerable Dr. De Rosette, a pupil of Cullen, a graduate of Edinburgh, and the Nestor of the profession, died in this city, April 1, 1859, at the advanced age of ninety-two. Dr. De Rosette states, when writing of the epidemic of yellow fever in Wilmington, 1800, seventy years ago, that no suspicion has been expressed by persons of any description of the importation or contagiousness of the disease, as its origin in every case could be reasonably ascribed to local causes. Drs. Hansford and Taylor, writing on the yellow fever of Norfolk, Va., in 1800, state that the epidemic of yellow fever had visited the seaports of Virginia, when there was no ground to suppose that it had been imported, and they are both fixed in the opinion that the fever of 1800 had an indigenous origin, and in this belief they claim to be supported by all intelligent persons (*Medical Recorder*, vol. iv., p. 206).

Wilmington, N. C., in 1862, *the Year of the Third Epidemic of Yellow Fever.*

What Malte-Brun says of Vera Cruz, applies well to Wilmington:

> La jolie ville Wilmington, Caroline du Nord, ne doit riens aux faveurs de la nature, des sables-arides et brulants, entourent la ville au sud, nord et est, tandis qu'on voit au ouest des marais des sèches (abandoned, uncultivated rice-fields).
>
> The pretty city of Wilmington, North Carolina, owes nothing to the favors of Nature. Arid and burning sands surround it to the north, south, and east, while to the west are only seen dried-up marshes (abandoned, uncultivated rice-fields).

To the east, back of the city, beyond the sand-ridges on which it is built, are extensive swamps, which are supposed to be a main cause of its insalubrity. The entire ground on which the city is built is composed of sand, underlaid in many places by a stratum of hard-pan, sulphuret of iron, sometimes called iron-stone; by the hard pan we mean condensed black sand, almost of the consistence of sandstone; in many places where the hard-pan is not met with, limestone-rock takes its place. The ground, except where it has been levelled for building, and laying out streets, is waving and undulatory, resembling very much the waves of the ocean, of which it, no doubt, once formed a part, producing, particularly in the suburbs, many slight hillocks and depressions. These depressions on the table-land, at an elevation of from thirty to fifty feet above the river-level, hold ponds and collections of water, which cannot soak through the soil, on account of the substratum of hard-pan below. Hence heavy rains sink not much lower than from eight to ten feet below the surface, and even much nearer in many places, even on the sand-hills, though, when the hard-pan or limestone dips deeper, the wells extend as low as from twenty to thirty feet. The site of the city is on a sand-bluff, elevated some fifty feet above the level of the Cape Fear River, a dull, sluggish, muddy stream, bordered on the western side, opposite to the city, by a continuous line of marshes, partly dried up, and partly covered by each tide, and by the overflow of the river during the spring and autumnal freshets. Truly, as Malte-Brun remarks, Wilmington owes nothing to the gifts of Nature, and yet there is not now a prettier city in the South. A century of hard, constant, and unremitting toil has clothed the streets and gardens with walls of living verdure. Long rows of stately trees now shade her wide and ample streets. Numerous elegant gardens perfume her atmosphere, and the extensive improvements that time has effected, now render this one of the most pleasant and healthy cities of the South. All of the swamps to the north, south, and east are drained. Comfortable dwellings now stand where in 1800 and 1862 only continued marshes and morasses were visible. With a perfect system of sewerage and drainage, we no longer dread the yellow fever, since, even if imported, it could not live long in an atmosphere affording no food for it to feed upon. The case occurring lately in this place originating in Charleston, and terminating fatally, with the characteristic bronze skin, was followed by no others. Our winters, spring, and fall are unrivalled. The season of the Indian-summer, in October and November, is charming beyond description, and only in the summer months of June, July, and August is the heat overpowering. Then it pours down with tropical intensity, averaging 90° in the shade, and 120° to 130° in the sun. This heat is tempered and rendered less oppressive by the southwest wind which blows steadily all day, cooling the air, until welcome night comes to the relief of the parched earth. I know of no more delightful place for the worn consumptive of the North than this pretty little city of the South during the spring, fall, and winter. Even in summer we no longer dread yellow fever, should it be brought in our midst. Sporadic cases do and will continue to spring up during the intense heat of our summers, but with the improved hygiene of our city, epidemics are no longer to be dreaded. Such is Wilmington in 1871; but I have to deal with Wilmington in 1862, then suffering from pestilence, want, and almost famine.

In the yellow fever of 1862 a long-continued range of tropical temperature preceded the outbreak. An unusually high degree of the thermometer, averaging 90° in the shade, and running up to 120° to 130° in the sun. An unusually high hygrometer, which continued and increased, exhibiting an almost saturated atmosphere. Heavy rains, and unusually high and distressing radiation. This great humidity was evident to all by the mildew on linen clothing, the injury done to cloth garments, and the mould on boots and shoes, harness, and all articles made of leather. Many cellars were half filled with putrid and stagnant water, particularly on North Water Street, where it splashed up, through the yielding joints of the floor, when walked upon. After and during the heavy rains of 1862 we saw men bailing water out of the cellars in Market Street, both on the north and south side, between Front and Second, in the buildings then occupied as a drug-store by Mr. Willis, and the adjoining shoe-store of Messrs. Bradley & Brothers, but to no purpose, as the water rose as fast as it was removed. These cellars have since been filled up to the level of the street, and are now perfectly dry. Easterly winds prevailed, and for a long series of days blew from that quarter before and during the epidemic, which were interrupted by calms. We did not once experience during that period the north winds, which usually succeed rains and tornadoes at this season, proving so refreshing and invigorating, by suspending for a time the too powerful influence of the sun in a sultry clime. Rains had been very frequent in August, September, and October, but, instead of cooling the atmosphere, it was observed that they were invariably followed by more intense heat. For more than two months we lived in an atmosphere heated above 90°.

Most of the stores on North Water Street were used as retail and wholesale grocery-stores, and the floors were elevated only the thickness of the joists from the ground, say twelve to fourteen inches. The leakage of salt-brine from pork-barrels, vinegar, liquors, and molasses, ran through the badly-jointed boards, and mixed with the water below, formed by the excessive, heavy, and long-continued rains, producing a compound and stench that were intolerable to the senses and pernicious to the health. Extensive diggings around the city, by which miles of earth were turned up to form intrenchments for military purposes, were going on continually. Privies, back-yards, and streets were in an intolerably filthy condition. Such was Wilmington in 1862, during, before, and after the epidemic. Can any candid reader deny that here were more than sufficient causes to produce yellow fever, combined as they were with the intense tropical heat, then raging like a consuming fire? Need we seek for any steamer to bring a disease, the seeds of which were already sown in our midst? During this condition of things the steamer Kate arrived August 6, 1862, but several deaths from yellow fever ensued previous to her arrival. So, even if yellow fever were on board, it cannot be said that she originated the disease.

Whether the steamer Kate had yellow fever on board when she arrived in Wilmington is a point we do not intend to discuss, or take upon ourself to decide. With a full consideration of all the circumstances connected with the beginning of the epidemic, we do not hesitate to affirm it as our belief that yellow fever would have visited this city, even if the Kate had never reached our shore. Dr. Nathaniel Hill, of this city, in reference to the fever of 1821 (*Medical Recorder*, vols. lxxx. and lxxxix.) writes: "I could not observe without conviction that my patients who sickened in the infected districts did not disseminate the disease. In several cases, however, five or six weeks afterward, other members of the family have sickened; but not referable surely to contagion, dormant for so long a time. Rather in conformity to the well-known laws of epidemic diseases, the epidemic had spread itself, and vitiated and poisoned our whole atmosphere." Dr. Hart, a contemporary of Dr. Hill in 1821, and then a resident of Wilmington, North Carolina, but at the time he wrote a practising physician of New Orleans, in a report made to the Medical Society of New Orleans, which report was unanimously adopted by that society, took the ground that yellow fever is not contagious. The great pestilence which raged at Buenos Ayres during this present summer is a striking illustration that this plague is peculiar to cities and towns. The mails just received from Buenos Ayres announce the continued and increased prevalence of yellow fever. The following is an extract from a private letter: "The yellow fever is taking the people off at the rate of two hundred and fifty a day; whole squares in the southern part of the city are abandoned. Strange to say, the harbor and shore are entirely free of disease. It is only the air of the city which seems to be poisoned, and this comes of its crowded population and overflowing, neglected water-closets and cess-pools." Strange how causes repeat themselves!—just as Wilmington was in 1821 and in 1862. This testimony is more valuable as it comes from an unprofessional man, who has no pet theory to sustain, simply a clergyman, a man of plain, practical common-sense, the Rev. Mr. Ashe, the chaplain of the British legation at Buenos Ayres. The city is said to have been reeking with filth, steaming like a dunghill, whenever a hot sun came off after a rainfall. There were no drainage facilities, and what wells there were in the city were utterly unfit for use.

This disease cannot be propagated from one locality to another, unless the air is suited to its production. Cases that were taken to Fayetteville, on the banks of the Cape Fear River, in the adjoining counties, or on the sounds, that were treated in private houses or public hotels, never in a single instance communicated the disease to friends, nurses, physicians, or servants.

I will here cite a few cases from my own practice and personal knowledge: Mr. Newhoff left Wilmington for Fayetteville with a high fever upon him early in the beginning of the epidemic. Before reaching Fayetteville, a town higher up the river, one hundred and twenty miles from the city, the fever rapidly increased, and ended fatally with black-vomit; none of the passengers or crew of the crowded steamer took the disease, nor did the parties who prepared his body for internment. Mr. William C. Bettencourt died in the village of Clinton, an interior town, fifty miles from Wilmington, of well-marked yellow fever, contracted in this city; and the disease did not spread to a single individual of that village. Mr. Gennings died at Durham Station, on the North Carolina Railroad, near the city of Raleigh, of yellow fever; and we have yet to hear of any case reported in Raleigh or its vicinity. On Wrightsville Sound, eight miles from this city, died many of our most noted and valued citizens, tenderly nursed by loving wives, children, and friends, Dr. Drake, Mr. Jewett, Mr. Lippitt, and others that I might mention if necessary. Not a single case was contracted from these parties. On Masonboro Sound, eight miles from Wilmington, during the epidemic, two families occupied the same house, a small two-story dwelling with a piazza on the front and rear, refugees from the pestilence in Wilmington. One occupied the lower part, the other the second story. With the occupants of the second story we have only to do. The gentleman in question, Mr. John Dix, an old and valued friend of the writer, after carefully removing his wife and three children from the dangers of the pestilence, imprudently visited Wilmington too often, and was taken with the fever after an exhausting day's work among the sick and dying of the city. I rode down to the sound at midnight, and found him calm, quiet, and composed, but throwing up large mouthfuls of the terrible black-vomit. He had nothing to do but to make his will, and died in two hours. Neither Mr. Chadbourne, the occupant of the lower part of the house Mr. Dix died in, and who witnessed his will, his family nor servants took the disease—no one on the premises whatever. Even Mrs. Dix, who tenderly nursed her husband to the last, and her three small children, escaped.

Not a single case occurred on these contracted premises then occupied by two large families, among the children, heads of the families, nurses, attendants, servants, or visitors, before, during, or after the illness of Mr. Dix.

The wife of Mr. John C. Bauman, a prominent citizen of this place, died early in the epidemic; all the family then removed three miles from the city into the country. Soon after a son of Mr. Bauman, who was exposed to the disease in the city, was taken sick with black-vomit, and recovered. No other member of the family suffered, or any of the country people in the vicinity. I might multiply instances, but these will suffice to show that the disease will not spread, unless it meets with an atmosphere suited to its propagation, but dies out with the death of the individual who carried the disease from the infected region. During the epidemic large quantities of salt were manufactured on the coast, giving employment to a great many persons, wood-cutters, salt-boilers, and teamsters. From Wilmington all their supplies were drawn, and, as the shipping-point, all of the salt was there carried, notwithstanding the fever. Free and constant intercourse between these two points, one open to the pure and healthy breezes of the ocean, the other lying in the valley of the shadow of death, not a single case of fever was contracted on the coast, nor did it spread from contact with those who, taking it in Wilmington, carried it to the sea-coast, and there recovered or died. The railroad-trains and river-boats continued to run, people from the adjoining counties came to market and courts part of the time. Of those who visited the city, some carried the fever home with them, but in no instance communicated it to others in the country. Can scarlatina, small-pox, or measles show a similar record? In 1862, after and during the yellow fever, auctions of furniture, bedding, bedclothes, and the effects of parties who had died of yellow fever, constantly took place in the city. On account of the scarcity produced by the rigid Federal blockade, they were eagerly bought without question, carried into the adjoining counties, and used daily and constantly; no ill result in any instance took place. Not a solitary case of yellow fever was reported, or heard of. Many persons have inhabited apartments in which yellow-fever patients have died, without their being whitewashed, cleaned, or fumigated; others have slept in beds on which such patients have died, without cleansing the bedding in any other way than exposing it to the sun and air. Others have used their clothes and linen, without any other purification than simple washing, and yet I know of no solitary instance in which the disease has been communicated in this way. During the war, after the epidemic of 1862, clothing, dry-goods, provisions, etc., etc., were daily lightered from vessels quarantined down the Cape Fear, and spread wide throughout the South by rail in every direction. No fever resulted from this great diffusion of cargoes. Military necessity has thus taught us a valuable lesson, which we have too soon forgotten. Dr. Thomas remarks in his review (p. 228) that, notwithstanding the constant rains in July, August, and September, in 1867, although fevers of a malarious type existed, and diarrhœa and dysentery were rife throughout the city, no indications of yellow fever manifested themselves. To this I could reply that we do not contend that excessive rains alone cause yellow fever. Along with the excessive rains there must be a high degree and prolongation of atmospheric heat, and other local causes of insalubrity, and those rains must be followed by most intense heat, as in 1862, instead of cooling the atmosphere, as in 1867. In 1867 the ponds were drained by the authorities as soon as formed, and great attention paid to the hygiene of the city. Not so in 1862. But I do not intend, or wish to dodge the question in this way, for in the proper place I will show, from the statement of my talented young friend Dr. Love, that one or more fatal cases of yellow fever with black-vomit did occur in 1867. In 1865, after the occupation of the city by the Federal army, the Confederate States stables were taken possession of by the army of invasion, and soon after, a large auction of condemned horses, mules, wagons, and ambulances took place, at the site of the pond described in the report of Dr. Wragg. At the head of Market Street, three squares north of the Marine Hospital, this sale was attended by me personally, in connection with a large number of the citizens. The large pond, described in Dr.

Wragg's report, was even then in existence, filthy, offensive, and slimy, being the natural basin into which all the refuse matter and dung from the high ground around the stables was carried after every rain. I am not mistaken in again asserting that this was the case in 1862, as then reported by me to Dr. Wragg. I think that my friend Dr. Thomas is in error when he asserts that the deposit from these stables was constantly removed by the owners of market-gardens in the vicinity of the city. The doctor must surely have forgotten that in 1862, the time I wrote, few, if any, market-gardens were in existence, that most persons cultivated small gardens attached to their own houses, and that only in 1865 and 1866 the extensive market-gardens he described sprang up. Our people, impoverished by the results of the war, sought new channels of industry, and tried to rebuild their shattered fortunes by the cultivation and shipping of vegetables to the Northern markets. Thus the extensive market-gardens around Wilmington arose, creating a demand for manures which did not exist in 1862. As a proof of this assertion, the Confederate Government constantly imported onions and other vegetables from Bermuda and Nassau, and I can well remember when the Confederate agent, Mr. Seixas, presented me as a great treat a few onions that had just run the blockade from Bermuda. Had the farms and market-gardens, that Dr. Thomas describes as removing the offal from the stables that polluted the pond at the head of Market Street, then been in existence, why would the government have imported vegetables they could have more easily procured at their very doors?

My learned friend surely confounds 1862 with 1865 and 1867. Our reviewer remarks (p. 235) that "we cannot remember that jaundice was so common, certainly not epidemic, as represented in Dr. Anderson's statement." There was quite a number of cases, but by no means as many as during the same season in 1865.

In this we are sustained by the physicians who were here, and engaged in practice both years. Now, let us look into this point a little in detail. When the yellow fever commenced here, and before, most of our physicians were absent with the army, doing duty either in the hospitals or in the field. Present in Wilmington at that time, Drs. Cutler, Dickson, Thomas, Love, Medway, Bowie, Ertkinker, and Anderson, of the regular faculty; in addition, Dr. Freeman, a homœopathist, with a very large practice, and Dr. Schonwald, an eclectic, also with a very large practice, both in the city and in the adjoining counties, ten in all. I have made special inquiry of all these gentlemen, now living or present in the city, as to the prevalence of jaundice before the advent of yellow fever. As I am disposed to do all possible justice to the opinion of a very eminent physician and warm personal friend, the reviewer, I will here state the evidence in a perfectly impartial manner, *pro* and *con*, giving the testimony on each side.

Drs. Thomas, Cutler, and Love do not recollect to have seen or treated an unusual amount of jaundice. Dr. Bowie states most unequivocally that he was struck with the large amount of jaundice prevailing among the troops quartered in the city and its inhabitants, and has often remarked to me that it looked very much to him that yellow fever was present, and this long before the Kate made her appearance. Dr. Bowie further states that, in conversation with the late Dr. Dickson, Dr. Dickson made the remark that jaundice was unusually prevalent, thus confirming the statement that Dr. Dickson (with whom I was unusually intimate) made to me, more than once, previous to his death of yellow fever. Dr. Ertkinker, who died after the yellow fever, of consumption, often alluded in conversation with me to the frequency of jaundice before the yellow fever appeared, and long before the arrival of the steamer Kate. In this statement I am supported by Dr. Freeman, with whom Dr. Ertkinker had similar conversations. Dr. Medway also alluded to the remarkable amount of jaundice, both to myself and Dr. Freeman. Dr. Schonwald has already given in his evidence, as to the prevalence of jaundice in Wilmington, in the report of Dr. Wragg, and has not retracted it so far. Dr. Anderson then bore most decided testimony to its being epidemic, and is still of the same opinion. Let us count up the testimony. Thomas, Cutler, and Love do not remember to have seen or treated much jaundice, and bear only a negative evidence, that is, they claim that in their own immediate practice, which, I most freely admit, is very large and extensive, not many cases of jaundice presented themselves. Now, in conformity with the universally admitted rules of evidence, if one hundred men state that they have not witnessed an event, it does not invalidate the testimony of ten who state that they have seen it. But among ten physicians then practising in this city, three—very eminent and impartial ones, I grant—only state that they have not witnessed much jaundice, while seven most distinctly and unequivocally assert that it was epidemic. The weight of the testimony is, I think, clearly on the side of the great prevalence of jaundice, as I lay much stress on this point, thinking with Drs. Bowie and Freeman that what we then supposed to be jaundice, was really yellow fever in disguise, a disease then personally known to none of us, but which we were all fated, in a terribly brief interval of time, to be too familiar with. I lay before the reader a letter of Dr. Buie to the writer, transcribed *literatim et verbatim:*

WILMINGTON, N. C., *August* 25, 1871.

DR. E. A. ANDERSON.

DEAR SIR: In reply to your inquiry in regard to the appearance of jaundice, during the summer of 1862. I distinctly recollect of having treated several cases in July, August, and September of that year, which were well marked, and from July to November there was a marked tendency, in cases of fever of any type, to become jaundiced. I saw a case in the Confederate Hospital, in this place, in July 1862, which was well marked. I had several cases of injuries from accidental wounds, the subjects being soldiers in apparent good health, that soon took on the bronze hue in the course of a few days after the receipt of these injuries. I saw yellow fever in Wilmington, and in a large proportion of the cases yellow fever supervened on attacks of the ordinary local fevers of this section. In fact, I learned from Dr. Dickson that the great trouble here was, that the cases became malignant when the patient seemed to be convalescent from the ordinary bilious and intermittent fevers. There was a perfect epidemic of jaundice in the army stationed here in 1862.

Yours truly,
D. M. BUIE.

The reviewer surely cannot dispose so slightly of the case of John Sutterloh, as reported by Dr. Schonwald. The doctor says positively that he had fever and jaundice (yellow fever), and died early in August, before the arrival of the steamer Kate, with black-vomit. The only evidence going to oppose this is that none of his friends say he had yellow fever, and one of his neighbors denies that he had. Who is the best judge, the doctor or the unprofessional attendant? The "Hole in the Wall," where Georgianna Weeks resided, is a house of ill-fame, lying one thousand feet, or more, to the southward of the railway, on which the Kate was hauled up for repairs. The wind blew steadily from the east all the time, and, of course, could have had no other effect than carrying off, instead of on, the "Hole in the Wall" any supposed effluvia or contagious matter from the Kate, since yellow fever, like other infectious diseases, usually travels with, and not against, the course of the prevailing winds. The cemetery records were so loosely kept that the error in date may be as well in our favor as against it. In the case of Mr. Morriss (which Morriss I cannot at the present time remember, it being now eight years since the epidemic), the facts of his case were dictated to Dr. Wragg by myself, then recovering from an attack of yellow fever, were fresh in my memory, not then being six weeks old. Surely this statement is more reliable than one written from memory alone seven years afterward. As an evidence of the loose manner in which the cemetery records were kept, I can cite the case of a lady, entered as dying of yellow fever, who had never seen or been near yellow fever, and whose death, of my own personal knowledge, was caused by childbirth.

The first entry in Oakdale Cemetery records, of deaths from yellow fever, is that of Esther E. Orrell, who died on the 14th of September, 1862. Now, if the yellow fever was brought here by the steamer Kate (as contended by the reviewer) on the 6th of August, 1862—and of course many deaths must have taken place before the 14th of September, a period of at least six weeks—why were they not reported as yellow fever, but entered as jaundice, consumption, hæmorrhages, malaria, intermittent and typhoid fevers, etc.? The Oakdale Cemetery records state that Georgianna Weeks died on the 23d of August, of hæmorrhage of the lungs. Dr. Schonwald says that she died on the 6th, of black-vomit.

Who furnished the cemetery records? Clearly the mistress or keeper of the brothel which this poor, unfortunate outcast died in, whose interest it was not to deter or frighten any visitors away from her establishment, in which an inmate had so recently died of a malignant and fatal fever.

The first notice of the presence of yellow fever in Wilmington is a publication of a letter from the late Dr. Dickson to the mayor of this city. This letter is dated the 9th of September, 1862, and published on the 17th of September, 1862, in the *Wilmington Weekly Journal*. In this paper Dr. Dickson states that he has had five cases, but hopes that it may not spread, as more attention is being paid to the cleaning of the city, etc.

The first mention I can find of any death from yellow fever published in the *Wilmington Journal* is that of the late Dr. Dickson, who died the 28th of September, 1862. It is true that numerous deaths were reported, the names of the parties given in full, etc., but the disease is not put down, except in this case. This was, no doubt, an inadvertence, and unintentional. The reporter of a paper only publishes what is handed to him, or what he collects. It is not his duty to sift out its causes, nature, or names of a disease, or any notice of a death that may be handed to him, but to print it as he finds it written. So also the keeper of the cemetery records; he enters the disease just as the friends of the deceased give it in. On the margin of the cemetery records there is a note, in which the yellow fever is officially announced as epidemic, Saturday, 13th September, 1862. Why, then, if yellow fever commenced by the arrival of the steamer Kate, August 6, 1862, and it was only admitted as present on the 9th of September (five cases of Dr. Dickson), and as an epidemic on the 13th of September—why, then, we again ask, were there no deaths reported before that of Mrs. Orrell, on the 17th of September, a period of over a month? Surely, if the Kate introduced this fever, more or less deaths must have happened from it before this time. The answer is plain: deaths did occur from yellow fever in July and August, before, during, and after the arrival of the Kate, but were not reported as such, being entered under different heads. The reason is obvious. Not a physician was practically acquainted with yellow fever. All we knew of it was derived from our books. And much of this was forgotten in the constant toil of our profession; and as we never expected to meet with this disease, we had no immediate necessity of keeping up our reading on this subject; hence we failed at first in recognizing the enemy. The friends of the parties deceased usually apply for a permit for interment, and hand in the cause of the death, or the name of the disease of which the party died, and it is thus entered on the cemetery records, and published in the daily papers. As I said before, in the commencement of an epidemic, few recognize a new and strange disease, and few are willing to admit the presence of the plague-fiend in their midst. Few are willing to believe that a near relative died in their own home of a disease that may carry them off next, or some other dear to their hearts. They hope on still, against hope itself; will not see or own the danger; and, willingly cheating themselves, thus cheat others. It requires much moral courage even in a physician to announce the presence of a deadly disease; he is ridiculed, scouted at, and called an alarmist. Hence in no place is the fever admitted to be epidemic, until it is impossible any longer to conceal its nature. This is the reason we find no records of deaths from yellow fever previous to the 17th of September, 1862, fully six weeks after the arrival of the steamer Kate, After a fever is acknowledged to be and is proclained an epidemic, the mortuary register and death notices in the daily papers are of great and acknowledged value. Before this I contend they are unreliable, and cannot be used or admitted as evidence. Mrs. Annie Crapon is recorded as having died on the 9th of September of bilious fever; and on the 19th of the same month, her husband, Marsden Crapon; his disease is not mentioned. Both these parties died of yellow fever; and yet it is not so entered on the cemetery-books. Louis Swartzman died on the 3d of September, reported apoplexy, certainly yellow fever, but not so entered, although it is claimed that the yellow fever was brought here by the Kate on the 6th of August, and raged already from four to six weeks. Of what value are records so loosely kept, where yellow fever is not even mentioned, until it could no longer be concealed? I cannot attach the importance the reviewer does to the case of Florence O'Donohoue. I saw him before any other physician, and regarded it then as a case of delirium tremens and fever, caused by exposure to the excessive tropical heat of the sun, increased by the still greater heat of the furnaces of the steamers. How frequently we see habitual drunkards throw up, and pass by the bowels, black matter, after a long and continued debauch! This I witnessed more than once in the case of a young German by the name of Albert Briersteine, in August, 1870, an habitual drinker, then attending the bar in Klein's Garden, in the northeastern quarter of the city. He had many of the symptoms of yellow fever, and passed black matter up and down, but recovered entirely. Had he arrived in a vessel from the West Indies, it would have been pronounced yellow fever, though there was not then or now any suspicion of it. O'Donohoue's death is entered on the cemetery-records as taking place on the 13th of August, and he was there stated to be crazy (delirium tremens). Surely if the reviewer can quote these records in his favor in one instance, why can I not in another, when they are in mine? though, from what I have stated above, I attach but little importance to them. The mere fact of other deaths occurring in this hospital does not show that he communicated the fever to its inmates, even if he had it, which is by no means proved; for, as we contend, the yellow fever existed all around the military hospital anterior to the arrival of the Kate.

And why should it not attack its inmates, even if O'Donohoue had never been brought there? As it is, he remained there but one day before his death. In this very hospital, and in the same room in which Gregory, Muse, and McEachem died in 1862, I saw die in September, 1864, a young soldier of yellow fever, who had no communication whatever with any suspected case of fever, and in conjunction with Dr. Joshua Walker, of this city, then a surgeon in the Confederate army, made a *post-mortem* examination of the subject.

This case will be reported more fully below. Dr. Thomas says that the next case, Dennis Mitchell, was well when the Kate arrived in Wilmington, and went to lodgings on shore, where he sickened and died. The Kate was on the railway not far from this house, two squares or 1,000 feet, or one-fifth of a mile; but the house lay to the windward of the steamer, and not to the leeward. The east winds prevailing at that time, the poison, whatever it was, must have travelled in the teeth of the winds, and not with it, to reach Mitchell. Which is the most probable, that Mitchell caught the fever from the Kate, then lying to the leeward, or from Rouse's Pond, then being to the windward of his lodgings? The doctor admits that Mitchell was well when he arrived; we contend that he caught the fever on shore, and did not bring it from the Kate. If intense heat of the sun and excessive humidity; if abundance of material, animal and vegetable, undergoing decomposition, existed; if personal and public hygiene were neglected, in 1862, to such an extent as to favor the production of this morbific principle, why may we not have looked with equal reason for the same poison in 1865, when almost every section of the city and its suburbs were occupied by sick and dying Federal soldiers, taken from Southern prisons, and when miasmatic fevers, jaundice, dysentery, and diarrhœa prevailed extensively?—(Thomas's Review, p. 249.)

A very pertinent question, requiring a somewhat extended answer. In 1865, when the Northern army reached Wilmington, few if any of our old physicians were in the city. Some were absent with the Southern forces; some had left, so as not to be present at the entrance of the enemy—not returning for some time, on account of the unsettled state of affairs, and the complete destruction of the railroads, which prevented travel of any kind. Hoke's army lay between Wilmington and the northern part of the State, desperately fighting and slowly retreating, as they were hard pressed by the superior and overwhelming masses of the Federal forces under Terry. The intermediate country swarmed with camp-followers, bummers, marauders, and rascals of all kinds, the scum of an invading force, making travel not only dangerous but impossible. As Terry advanced and Hoke retreated, the invading army repaired the Wilmington, Weldon, or Great Northern railroad in a temporary manner. This road they kept for some time in their own hands, using it for the transportation of military stores, wounded, sick, and disabled soldiers, and recovered Federal prisoners. They seldom, if ever, granted passes to citizens to travel on the railroad; and hence Dr. Thomas was unavoidably detained from the city for some time. Present in Wilmington at the entrance of the Federal army, Drs. Medway, Love, Ertkinker, King, and Anderson. My office adjoined that of Colonel Randall, the provost-marshal, and thus I had full opportunities of witnessing what steps were taken for the cleansing of the city. I am somewhat diffuse, that the reader may appreciate the value of what I here state, and the sources of my information. The Federal soldiers were terribly afraid of yellow fever, and hence gave the very greatest care to the hygiene of the city. The provost-marshal and his assistants had a hundred negroes constantly employed cleaning the streets and sewers. Martial law prevailed; an unlimited supply of wagons, laborers, men, and money was at the disposal of the provost-marshal, and used unstintingly. All of the resident physicians were placed on duty in the Federal hospitals; and I was ordered by General Hawley, then commander of the city, to organize a hospital for the sick and dying Federal soldiers, taken, as Dr. Thomas forcibly expresses it, from Southern prisons. The hospital I organized contained from six to seven hundred patients, and was in the building, and in tents around the building, known as the custom-house, in the eastern part of the city, corner of Orange and Tenth Streets. There was also another large hospital in St. James's Church; a third in the Hill Building, corner of Mulberry and Fourth Streets; a fourth in the Wright House, a large building on Market Street; a fifth in the McRae House, also on Market Street; a sixth in the house now occupied by Adrien & Vollers, on the corner of Front and Dock Streets; a seventh in the very large building used as the Seamen's Home, occupied at the entry of the Federal army as a Southern Hospital; an eighth in the present Marine Hospital; a ninth over the railroad, on the property known as the Dickinson's property, overlooking the Charlotte Railroad depot; a tenth was a Negro Hospital, under the charge of Dr. King, of this city, at the northern end of

Front Street—a very large four-story building, now occupied as offices by the Wilmington & Weldon Railroad Company. Kidder's Mill, a very large building, with its numerous outhouses, made an eleventh hospital. Dudley's Mill, a structure with out-buildings similar to and of equal capacity to Kidder's, made a twelfth hospital. This was in charge of Dr. Tucker, of the United States Army, as a Colored Hospital, and had at this time three hundred patients. John C. Heyer's house, corner of Fourth and Walnut Streets, made a thirteenth. The very large four-story house of Benjamin Berry, corner of Nun and Second Streets, was occupied by Dr. Savage, of the United States Army, as a hospital, making the fourteenth. The depot of the Manchester Railroad, across the river, opposite the foot of Market Street, was the fifteenth hospital, used at that time for negro refugees. The large side-wheel steamer Spaulding, of one thousand tons capacity, lay at the foot of Orange Street, and was fitted up as a floating-hospital in the most complete manner, making sixteen hospitals in commission at the same time—surely enough for any given amount of sick and dying Federal soldiers. No army in the world for the last century has seen similar hospitals, not even in the Crimea, or Franco-German War, for their comforts and arrangements were admirable. Nothing was wanting, not even luxuries. The Christian Association supplied most liberally every want the government had overlooked. I had only to sign an order to get whatever I wanted for my sick. Canned fruits, soup, vegetables of all kinds, milk, preserves, fine wines, liquors, brandy, ale, and an unlimited supply of ice. Fresh loaves of bread, equal to any in the world, were served daily to the sick and convalescents by the government bakers; and a large California steamer, the Black Warrior, ran weekly between this city and Baltimore, bringing live cattle on her spacious decks for the use of the troops and hospitals. With sixteen hospitals at their command, attended by Southern physicians familiar with diseases of the climate, and an able corps of army-surgeons, with a trained body of nurses and hospital stewards, all under admirable military discipline, what was to be dreaded even if every section of the city was occupied by sick soldiers?—for the provost-marshal spared neither time, trouble, nor money to keep the city in good order, and it was so kept. Besides this, I had orders to send off, as rapidly as they could bear transportation, all of the sick and convalescents to Annapolis, Maryland, there to recruit in a colder and healthier climate. Large steam transports were at the disposal of the medical board, and used so efficiently that these sixteen hospitals were in succession rapidly broken up, so that by the month of June only two remained in commission, namely, the Negro Hospital, under the care of Dr. King, and the Sherman Hospital, under my care; these two soon followed the fate of the others, and by July, the hottest month of the year, no sick remained in Wilmington, save those belonging to the army of occupation, composed of three regiments, or about two thousand five hundred men. This is the reason we had no yellow fever in 1865, except the cases to be reported farther below. Yellow fever had nothing to feed upon, and, although sporadic cases appeared, it spread no farther. We will now enumerate the cases of yellow fever that have occurred in our practice and in that of our professional brethren since 1862, and up to the present time:

On the 9th of October, 1863, I was called to see I. Leslie, a bootmaker, from forty-five to fifty years of age, a man of very temperate and steady habits. Leslie resided in the northeastern quarter of the city, fully a mile from the river, in a dry, elevated situation. A regiment of Senior Reserves was quartered on the Westhall place, an abandoned country-seat about two miles from Wilmington. Westhall lay to the northeast of Green's Pond, an extensive sheet of water, on the eastern suburbs of the city. This pond was enlarged, and very much extended, by a new dam thrown up in continuation of the old one, widening the pond to the north and east. In this way a large mass of fresh earth was turned up, and exposed to the burning sun of October, which also acted upon the large sheet of water forming Green's Pond. About this time, in 1863, as is usual in our summers and falls, the winds blew steadily from the southwest, thus bringing the exhalations from the new dam and Green's Pond to the regiment of Senior Reserves, who were encamped in the direct track, or to the leeward of the prevailing winds. Leslie being a very trusty and intelligent man, held the responsible position of sergeant-major to the regiment, and was necessarily very much exposed in the discharge of his military duties. During the time the regiment was quartered at Westhall, he occasionally visited his family in the city on leave of absence, but never once went to the river-side, and, even if he had, could have done so with the same impunity as the entire population. No infected vessel then lay at our wharves, and no suspicion of yellow fever in our port. On the 9th of October, 1863, Leslie was brought in an ambulance to his house, two miles from the farm of Westhall, where his regiment was encamped. As his usual medical attendant, I was summoned to visit him, and found him suffering from the following symptoms:

1863.

Leslie complained of great lassitude, dull and erratic pains and rigors, heat and headache, thirst, and intolerance of noise and light, irregular pyrexial exacerbations and remissions, and great debility. The debility increased, the eyes became very yellow, bloodshot, and glassy, the skin also became of a yellow tinge, and covered with a cold perspiration, with sordes on the teeth, chapped lips, and dark, dry tongue, hurried respiration, great irritability of the stomach, hiccough, hippocratic countenance, difficulty of swallowing, and slow, stertorous, and convulsive respiration. The pulse was small and intermittent, urine blackish when drawn off by the catheter. *Subsultus tendinum* soon came on, rattling in the throat, and cold, clammy sweats. The skin became of an intense yellow hue, deepening rapidly after death, which was preceded by black-vomit, and black dejections taking place in four days from my first visit. Can any doubt that this was a case of sporadic yellow fever?

1864.

In August, 1864, I was called to see Jane, a colored woman, aged about forty, a cook in the family of one of our blockade-running merchants, then residing in the northwestern quarter of the city, near the Methodist Church, and found her laboring under the following symptoms: Rigors, succeeded by violent fever, and pain in the head and back. Pulse full and irregular, quick and tense, 90 a minute, eyes inflamed and of a yellowish cast, red and bloodshot, injected, brilliant, fiery, and glassy. Skin hot, dry, and pungent, though at times clammy, moist, and sweating, and followed by profuse perspiration. All of the well-known symptoms of yellow fever were present except black-vomit, which did not come on, as this patient happily recovered.

In August, 1864, in company with Dr. Joshua G. Walker, of this city, then in the Confederate States Army, I visited a young soldier who was brought up from the sound, eight miles from Wilmington, where there was no yellow fever or suspicion of it, since no vessel from a foreign port could possibly enter this sound, on account of the shallowness of the water on the bar, four to six feet. This young soldier, apparently twenty years old, was lying in the northeastern room of the house now occupied by Messrs. Adrien & Vollers, wholesale grocery establishment, corner of Dock and Front Streets. The house in question was then used as a hospital by the Confederate military authorities. When Dr. Walker and myself entered the room, the pillows, sheets, and floor were stained by black-vomit, and while standing by his bedside he gulped, or rather spouted up, a volume of black-vomit, without effort or apparent volition, and this so suddenly that I had to jump on one side to avoid being spattered. This poor, unfortunate boy died soon after, his body turning of a deep-yellow hue, and presenting all the unequivocal symptoms of yellow fever.

In September, 1864, Dr. Walker invited me to visit, at the Marine Hospital, a young soldier under his care, whose symptoms were very suspicious. As the doctor was not in Wilmington in 1862 during the epidemic, he had never treated yellow fever, or seen much of it, and therefore requested my opinion of the case. This patient had all the marked symptoms of yellow fever, which it is unnecessary here to enumerate, and died the same day. A *post-mortem* examination was made by Dr. Walker and myself, and from the contents of the stomach we were both satisfied of its being a genuine case of yellow fever.

1865.

June 27, 1865.—At the house of Mrs. Van Sycle, in the northwestern part of the city, near the Methodist Church, one of the most healthy and elevated quarters of Wilmington, I was called to see Charles Howard, black, aged about twenty-eight, formerly a slave, employed in a rice-field. This patient had suffered very much from want, privation, exposure to the rain, and hunger, before I was sent for. Found him suffering from rigors followed by high fevers, pain in the head and back, quick pulse, over 100. Skin hot, dry, and harsh, followed by profuse perspiration. Tongue thin, slimy, white, and moist, clean red edge and tip, great intolerance of noise and light. Eyes red, yellow, bloodshot, and glassy. (Being a coal-black negro, the color of the skin was not perceptible.) Wandering pains like rheumatic fever, great anxiety, with sickness and pain in the stomach, attended with convulsive vomiting, which was scarcely relieved by any medicine; urine scanty, and when drawn off by the catheter quite black. Extreme tenderness over the whole of the abdomen. Great and rapid prostration of strength, and burning thirst. The matter thrown up at first consisted of the contents of the stomach, of glairy mucus, mixed with bile; this ceased, and, as the disease advanced, was replaced by a large discharge of black-vomit, which took place by jets, and by a spasmodic contraction of the stomach, seemingly beyond the control of the patient. This same black matter was passed in quantities by the bowels. Soon after the black-vomit set in, the patient died, on the fifth day from my first visit.

Through the kindness of my friend Dr. Bowie, of this city, I am indebted for the brief notices of the following cases that he had seen in 1865. Dr. Bowie resided in the city during the terrible pestilence of 1862, suffered from yellow fever himself, and is in every way a most competent judge of its nature, being in every respect a most intelligent and reliable gentleman and physician. Dr. Bowie reports the cases of two children of Rufus Galloway, in the northeastern part of the city, in August, 1865, with bronze skin and black-vomit. Also the case of Mr. Evan Bevan, in the Bates House, corner of Fifth and Market Streets, in July, 1865, with all the symptoms of yellow fever. On the 31st of July, 1865, a child of Dr. Bowie had bronze skin and black-vomit; this child died. The other three recovered, making four cases in all in Dr. Bowie's practice in 1865.

1870.

August 18th.—Peggy McIlhenny, aged seventy-two, negro house-servant, living in the central part of the city, was taken ill with all the most prominent and well-marked symptoms of yellow fever, with black-vomit, and died on the fourth day. The black matter was also passed from the bowels.

On the 21st of August I was called to see John Hewett, then on a visit from Brunswick County to some of his relatives, who resided in the southeastern part of the city. The patient in question, as well as his friends, belonged to the middle class, honest, and respectable people, but very poor, indeed often in want of the ordinary necessaries of life to such an extent that, in addition to gratuitous medical attendance, I had to supply the medicines required. This patient was a young man about twenty-five years of age, very sallow, cachectic, thin, and emaciated, badly nourished, and of poor physique. He had all the characteristic symptoms of yellow fever, followed by black-vomit from the mouth, also from the bowels. He was one of the very few that I saw who recovered after this terrible symptom, but his recovery was slow and tedious, followed by œdematous swelling of the feet and ankles.

August 31st.—This is to me the most interesting case, presenting, as it does, strong and unequivocal symptoms of yellow fever, with black evacuations both from the mouth and bowels. Still more interesting, as the patient was the only brother of the writer who remained unharmed in the city during the entire prevalence of yellow fever. Strange, then, he should have escaped to have so nearly fallen a victim to a sporadic attack in 1870, eight years subsequently!

James Anderson, aged fifty-eight, commission-merchant. His attack was preceded by days of indisposition, with pains of a vague character, resembling more weariness in the limbs than actual pain. Sleep interrupted and attended with painful dreams and frequent and troublesome eructations. The day that Mr. Anderson yielded to the disease his strong will had kept off previously, he had gone into a warehouse that had been closed for a long time. The floor of this warehouse was rotten and close to the ground. A strong and offensive odor overpowered him, producing *malaise*, headache, nausea, faintness, and rigors, with a sense of weariness in the small of the back, and soreness in the muscles of the leg. When he came home and took to his bed I was sent for, and saw him immediately. Found my brother with a high fever, pain in the head and back, pulse full, 100 a minute. Eyes inflamed, and of a yellowish cast, red, injected, and bloodshot. Face flushed and purplish, looked like a man who was intoxicated (though Mr. Anderson is remarkably abstemious), countenance vultuous and shining, of a dark crimson-and-violet hue. With the exception of the yellowness of the eyes, there was not much jaundice on the body, trunk, and arms. Tongue at first moist, and covered with a white cottony fur, red on the edges and apex, but as the disease advanced it became dry, shrivelled, rough as a file, and red as if roasted, with a darkish-brown fur in the centre. Petechial spots made their appearance on the arms, chest, and abdomen of a red color, without elevation or desquamation. Urine natural and limpid; thirst moderate at first, increasing in violence as the case advanced, and finally intense, particularly when the perspiration was profuse, as it became in the latter part of the disease. Respiration was free and easy during the whole stage, and much less embarrassed than in ordinary fevers. This fever continued for seven days, and was slight and occasional for fourteen more, was complicated with dysentery and severe tormina, yielding only to opium, and astringents being prolonged to the 1st of October, or one month. Strange to say that, although all of the well-marked symptoms of yellow fever presented themselves, no suspicion of the nature of the disease was felt until black-vomit set in, which made its appearance on the fourth day. At first the matter thrown up consisted of the contents of the stomach, a glairy mucus, mixed with bile; this ceased, and was replaced as the disease advanced, with a discharge of brown, blackish, or chocolate-colored flakes, resembling coffee-grounds, which was gulped or spouted up without apparent effort or volition. The stools became tarry, dark, purplish, and bloody; the abdomen was soft, and the dejections resembled powdered charcoal floating in a serous fluid, or like thin pitch. The black matter was occasionally in the form of figured stools, or composed entirely of it, made into form and consistence by a small quantity of intestinal mucus. When this ceased it was followed by dysentery and severe tormina, which was only relieved by opium and astringents; to the dysentery succeeded obstinate diarrhœa of a mucous character. During the progress of the entire attack, the mental faculties preserved their integrity unaltered. My brother talked much of his expected recovery, was with great difficulty kept in bed, wished to get up and pursue his business, dress, and wash his hands at his wash-stand, being only pacified when the basin was brought to his bedside. Muscular strength was preserved to a great degree throughout the attack, and only failed when dysentery set in. He generally rose without assistance and walked unsupported to the water-closet. After his unexpected and almost miraculous recovery (for it was the worst case I have ever seen or treated that did not die), œdematous swelling of the feet, ankles, and legs came on; and it was many months before he regained his usual strength and health.

I will now report the cases kindly furnished me by my friend Dr. William I. Love, in the very words of that young, talented, and able physician:

In 1863, the physicians of this city, at the solicitation of the mayor, engaged to report to him any case of disease of a malignant character which might become epidemic, as soon as it should be recognized. Accordingly, on the 13th of October, 1864, three days after the appearance of white frost, I notified Mayor Dawson that three cases of yellow fever had occurred in my practice. They appeared about the same time, and terminated fatally within eighteen hours of each other, one on the night of the 12th, the others on the 13th of the month. The fever was of about three days' duration, and was followed by jaundice, collapse, and black-vomit. Two were on Princess, between Second and Third Streets, in a house adjoining the military prison. Many thought the disease was contracted by opening a trunk of clothing which had been used by the father of the family, who died here of yellow fever in 1862. The trunk had been carried to Kenansville, Duplin County, in March, 1863, and was there exposed to sun and air nearly every fine day during the rest of the year. When the family removed to Wilmington, the trunk was brought with them, and the airing was continued at intervals until the occurrence of the cases in question. I was disposed to attribute the origin of the disease to the privy of the prison. Its contents, rising several feet above the ground, oozed through the wall separating the two lots, against which the privy was built, and in this manner, as well as through an outlet made by the displacement of a few bricks, escaped into the back-yard, an enclosure some twenty feet square, bounded on the west and north by a compact brick wall twelve feet high, and on the east and south by the kitchen and dwelling. Several attempts were made by the family to close the aperture by having trash piled against it, a procedure apparently tending more to conceal and increase than to mitigate the evil. The bedroom of one of the victims was on the first floor, and opened by a window directly upon this effluvious space. The third case was that of a young mulatto woman, from Fayetteville, servant in a boarding-house on Market Street, about one hundred yards due south of the site just described. From the 28th of October to the 3d of November, in consultation with Dr. Devane, of Magnolia, I attended in that place (forty-eight miles distant by railroad) a young gentleman who resided and fell sick in Wilmington. He was treated here for bilious fever by one of our oldest and most accomplished physicians who practised largely in the yellow-fever epidemic of 1862. Immediately upon the subsidence of the fever, at the end of the third day, he was rapidly carried in an ambulance-car to Magnolia, and in a few moments after his arrival was seized with black-vomit. He had previously become jaundiced. After a tedious illness, recovery took place.

In June, 1865, about the middle of the month, a white woman from the country, young, robust, paramour of a military officer, died after a sickness of four days, on Front Street, east side, between Church and Castle Streets. The symptoms were mainly high fever, headache, delirium which continued to the last, nausea, vomiting, bowels responding sluggishly to purgatives, followed on the evening of the third day by remission of fever, hæmorrhages, suppression of urine, and collapse. At first I supposed the attack to be meningitis, but as the case became developed, I was firmly impressed that it was yellow fever, and employed, at my own expense, a trustworthy woman to remain in the room during the last stage, with instructions to keep for my inspection whatever might be vomited. To my surprise the patient vomited no more after the subsidence of fever. A *post-mortem* examination was held two hours after death, in the presence of Dr. Flanner, of this city, and two medical officers highest in rank of the army which then occupied this section. The stomach was found full of black-vomit, and its mucous membrane congested with numerous black points attached. Some of the dark fluid was carried to the office of Dr. Anderson, and there inspected by several medical gentlemen, among them Drs. Anderson and Greenhow, all of whom pronounced it black-vomit. No similar case came under my observation during that year.

July 20, 1869.—A patient of mine died on Sixth Street, between Dock and Orange, after an indisposition of not quite four days, characterized by the usual symptoms of fever, which subsided on the third day, and was quickly followed by black-vomit in large amount, which escaped apparently almost without effort. There was some diarrhœa throughout. Patient had been much exposed to the sun, and had eaten largely of wild-cherries the morning of the day he fell ill. There was a very slight yellowness of the skin, which was not observable till after death. No *post-mortem* examination of the viscera was held (patient's father died of yellow fever in 1862). A report of the case having been demanded by the mayor, it was furnished, and at my instance was referred to the medical society of New Hanover County, who appointed, as a committee of investigation, Drs. Anderson, Buie, and McRee. The two former were of opinion that the patient died of yellow fever, the lattor that it it was bilious fever, for the reason, as I understood it, that there was no evidence of importation, and that he was convinced yellow fever could not originate here. He visited with me an Irishman of spare habit, temperate, living in the same house where the suspicious case occurred, who had high fever for three days, which terminated with copious diaphoresis, and was followed by speedy convalescence. Dr. McRee pronounced it bilious fever. I expressed no opinion. In an epidemic of yellow fever it would have passed as an instance of the disease ordinarily, as bilious fever. The committee determined to make no report, as there was no indication of an epidemic of yellow fever, and therefore no occasion to alarm the citizens. Remarkably cool weather for the season prevailed during the three or four weeks ensuing, and it was very hot no more that summer and fall, although previously it had, during a period of six weeks, been unusually so.

In 1863, Dr. Anderson reports 1 case	1
“ 1864, Anderson, 1; Walker, 2; Love, 4	7
“ 1865, Anderson, 1; Bowie, 3; Love, 1	5
“ 1869, Love, 1	1
“ 1870, Anderson, 3	3
Cases reported and occurring since 1862	17

The population of the city in 1862—owing to the large numbers that fled when the disease was pronounced epidemic, those absent at the sea-coast making salt, those employed in military purposes outside of the city, or engaged in the army, or refugees in the interior towns—hardly amounted to four thousand; the number of cases to fifteen or sixteen hundred, and deaths about five hundred; that is to say, one out of every three of the population was attacked with the fever, and of the cases one-third proved fatal, which Dr. Wragg justly remarks exhibits a very great mortality.

On the treatment of this disease I will not dwell, nor more in detail of the ravages it caused. This has been more eloquently and ably described by Dr. Wragg. I will only say that we had as able a corps of physicians as the country could have produced, with one eclectic and one homœopathic practitioner. All modes of treatment were tried. Quinine in twenty- and thirty-grain doses, sweating, calomel, digitalis, veratrine, muriated tincture of iron, wrapping the entire body in wet sheets, together with the expectant or do-nothing plan; or simply nursing. Among the volunteer physicians were the ablest from the hot-beds of yellow fever, Charleston and New Orleans; and from Richmond, Virginia, also, a very able physician, formerly of the Northern navy, a man of profound observation and great experience. I refer to the late Dr. Greenhow, who had witnessed this disease on the coast of Africa and Brazil. Truth compels me to say that one mode of treatment was as successful as another. As in cholera and diphtheria, most of the bad cases died under any or no treatment, while the milder recovered. After the lapse of more than a century, cholera, diphtheria, and yellow fever, continue to be the *opprobia medicorum.*

And now we must bring to an end this somewhat too lengthy article; what we have written has been penned in no captious spirit, with no desire for victory, with no unkind feelings toward our reviewer, with whom we have been for many years in the most intimate relations, and for whom we entertain the very highest regard as a physician, gentleman, man of science, and a thoroughly high-minded practitioner. We were most unwillingly drawn into this controversy, for, as Dr. Wragg has not thought proper to reply to this review, the task has fallen upon myself especially, as I furnished most of the controverted facts of his report. Whatever may be the result, I will not take it up again, being more than repaid if I have elucidated a single truth, or divested the minds of the coming race of young physicians, who will shortly succeed us on the stage, of the dread of contagion, or the idle and unmanly fear of this fever.

Art. II.—*On the Treatment of Syphilis.* By Alfred Fournier, M. D., Physician to the Lourcine Hospital, Paris. Translated by R. W. Taylor, M. D., Surgeon to the New York Dispensary, Department of Venereal and Skin Diseases.

PART II.—ON THE TREATMENT OF SECONDARY SYPHILIS BY THE METHOD OF SUCCESSIVE TREATMENTS.

I will not enter into the details of every method of administering mercury, nor will I describe every pharmaceutic preparation, as these subjects are thoroughly treated of in works upon materia medica. It is simply my purpose to describe and to study the merits of the principal methods of treatment to which observation has given the preference.

The external method, or that by inunction, is the oldest and the most active. It is this method which in the sixteenth century excited that enthusiasm of which the writings of the old authors so plainly attest. It was this method which first afforded beneficial relief to the French disease, and it is to the early observers of this disease that the honor of having discovered, and of having called attention to its wonderful efficacy, is due. It consisted then, as it still does in our day, of a series of daily inunctions upon various portions of the body, with a certain quantity of mercurial ointment. The method, however, has undergone improvement and modification in some of its details.

It is an undoubted fact that the inunction-cure constitutes the most energetic method of administering mercury, while at the same time it is the most sure and rapid in its effects. Opinion is almost unanimous upon this point, and I can say for myself that on very many occasions I have seen this treatment (at least as regards its immediate results) produce the most striking success. Still we do not employ this method at this hospital, or at least we only resort to it exceptionally. The reason is, that it is attended with two drawbacks, or rather with a drawback and a danger. The drawback is, that it is a dirty and disagreeable treatment, which ruins the clothes, is very unpleasant and disgusting to patients, and it causes them to dread it. Women are especially stubborn as to this treatment, they submit to it with repugnance, and very soon implore you to substitute another treatment for it.

The danger of it is stomatitis. Unless we take care, mercurial inunctions scarcely fail to cause a violent inflammation of the mouth. Only a few days elapse before the gums are affected, and, if the treatment is then persisted in, it may produce instances of those frightful stomatites of which the works of our forefathers have left us such lugubrious tableaux—stomatites which tumefy, soften, and ulcerate the gums, which cause the teeth to loosen, and which may even attack the bone, and which always entail upon the patient several weeks of torture.

Under these circumstances, then, it is necessary in practice to always bear in mind the two following propositions:

1. That, of all methods of administering mercury, that by inunction is the one, without doubt, most liable to the dangers of stomatitis.

2. That the stomatitis induced by inunctions is generally more rapid in its development, and at the same time is more intense and serious, and less susceptible of being controlled, than is the form which is produced by the internal and other methods of using mercury. Whatever we may do, we always run a certain risk; whatever attention and watchfulness we may exercise in our treatment, we are never certain that we shall not produce salivation by this method. Therefore, when we are forced to have recourse to it, we guard ourselves with many precautions, as follows: by prescribing a single inunction every other day of from thirty to sixty grains of strong mercurial ointment, or even three inunctions on three successive days, followed by a rest of four days; in administering chlorate of potassa internally, and in the form of gargle; in advising our patients to take scrupulous care in cleansing the mouth, and especially in watching every day the state of the gums, in order that we may suspend the treatment the moment any inflammation threatens. Still, in spite of all our care, we are never certain that we are safe from an attack of stomatitis. So, according to my views, inunction ought never to be used as a routine treatment, but always as an *exceptional* method, which is reserved for special cases, and only put in practice in accordance with either of the three following indications:

1. In cases of serious lesions, which it is necessary to act upon surely and rapidly.
2. In cases which are rebellious to other methods.
3. In cases in which mercury is not tolerated by the stomach.

A second mode of external administration of mercury is by hypodermic injections. This treatment of syphilis has been too recently introduced to allow us to pass a final decision as to its merits. However, we certainly can foresee that it will not replace the inunction-cure, nor especially the internal method of using of mercury. This method has not in general practice answered the expectations which were entertained for it, nor has it warranted the premature praise which, at its introduction, was lavished upon it. For my part, I have not, as yet, derived any thing satisfactory from it; and, although my observations are not sufficiently numerous to absolutely condemn it, I think I am warranted in criticising it very severely.

In the first place, it is an impracticable method, as it necessitates every day one or two operations, and of course a daily visit from the physician to the patient, or *vice versa.* Then, again, it is not free from dangers and inconveniences. When I assumed my service at Lourcine Hospital, I saw several women who, as a result of these injections made upon the back, showed abscesses which involved the whole thickness of the skin, and not one of them measured less than an inch and a half in diameter. Since that time I must acknowledge that the method has been rendered more perfect, and it is said that these abscesses are not to be feared now. But, although I have followed the most approved methods, and have used the improved instruments and formulæ, I have produced, very often, painful and hard subcutaneous tumors of the size of a walnut, or even of a small egg, and these tumors remained several weeks. I have also noticed that upon the regions where many of the injections have been made, at least upon women, the tissues have become remarkably painful and sensitive to the least pressure or touch—so much so that several of our patients could only walk with great difficulty (these were cases in which the injections had been made on the lower extremities), and others could not lie down, in consequence of the injections having been made upon the back, or on the scapular region. I must also say that our patients evince a great repugnance to the treatment, and several would have left the hospital if we had not changed it.

Hypodermic injections, then, will probably be retained in practice as an *exceptional* method, to which, in *absence of a better one*, we shall have recourse in certain cases, as, for instance, when mercury for any reason cannot be given by the stomach. I certainly believe that it will never be accepted as the routine method, for, without passing judgment upon its therapeutic value, it involves in its application such serious inconveniences.

The method of treatment by *ingestion* of mercury is the one followed by the majority of physicians, and consists in its administration by the stomach in various forms, and is very simple in its application. This method is generally well borne by patients, and is infinitely less dangerous as regards the mouth than the external methods, and it generally yields very satisfactory results.

It would be unnecessary to mention all of the mercurial preparations which have been used in this way, as most of them have passed from use, and do not present properties which are not found in those ordinarily used. Of all these remedies, only two are worthy of our consideration, and to them physicians justly give the preference. They are the bichloride of mercury and the proto-iodide of mercury.

Since the time of Van Swieten and Dupuytren the bichloride of mercury has been extensively used in the treatment of syphilis, and it really is a very active remedy, and does not enjoy an undeserved reputation.

Unfortunately, it is frequently not well borne, and particularly by women. In the first place, its unpleasant taste prejudices them against it. It is true that we can generally avoid this by prescribing it in the form of pills; but in whatever way it is administered it always produces trouble of the stomach, such as cramps, gripings, and colic, so much so that in the hospital here it is known under the *sobriquet* of *chest-breaker* (*casse-poitrine*). I should also add that its prolonged administration is followed by dyspepsia or a persistent gastralgia. It is a remedy which, as a general rule, is badly borne by delicate stomachs, and it does not agree with women, and I have also observed men who have suffered similarly from its use.

The proto-iodide of mercury is by no means subject to the same objections. It is a comparatively mild preparation, and is easily assimilated by the stomach, for eight or nine times out of ten it does not occasion any gastric disturbance even in women, especially if it is carefully used and combined with a small quantity of opium. It is generally given in the form of pills, and its daily active dose for an adult woman is from five to ten centigrammes.[1] But there is no absolute rule, for the individual susceptibility is very variable, and in the first place the *tolerance of the stomach* of one patient differs from that of another.

In the second place, it is also important to determine the *tolerance of the mouth*, as that also varies in different persons. However, we can state that the medium dose for a woman, which will not affect the teeth, is five centigrammes; that a dose of ten centigrammes sometimes, and even frequently, irritates them slightly; and that fifteen centigrammes and above that scarcely fails to act upon the mouth.

In the third place (and to this the theoretical dose is always subject), there is the *therapeutic effect* produced by the medicine. If a given dose produces a marked influence upon symptoms, it is good, whatever it may be, and there is no necessity of increasing it, at least during the time it preserves its effect. Thus, although five centigrammes are generally sufficiently powerful for women, there are many in whom this dose does not produce any effect, and for whom larger doses are absolutely necessary, and very often I have cured, with daily doses of fifteen and twenty centigrammes, symptoms which have resisted a milder dose.

It is necessary, then, to know the *dose which is tolerated* and the *active dose* (and this unfortunately is not always identical), to test the susceptibility of each patient, and treat them in accordance. Nothing can be foreseen or determined by rule *a priori*, as every thing depends upon the indication of the case, and in short *the dose of the remedy remains always* and of necessity subject to three conditions: *gastric tolerance, tolerance of the mouth, and the therapeutic effects.*

With a little practice and attention, the physician soon overcomes these difficulties. An embarrassing case occurs sometimes in which there is an inequality between the dose tolerated and the active dose, or, more simply stated, when the mouth and stomach will not tolerate a sufficiently large dose to effect a cure; but these cases are happily very rare, and it is in them that we have need of skill and of expedients in order to manœuvre between the two dangers. We have here no rules laid down in advance. Sometimes it is well to change the mode of administration of the remedy; sometimes it will be necessary not to change the method, but only the remedy itself; at other times it is well to *force* tolerance by associating with the mercury a full dose of opium, or of combining it with the chlorate of potassa, even by adopting, as I have sometimes done with success, the interrupted treatment, which consists in administering the mercury for some days in full doses, then suddenly ceasing, and then resuming again. These, however, are details of only secondary importance, which need not be further pursued in a general view of the treatment of syphilis.

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[1] In estimating the equivalents between French and American weights, it is necessary to remember that one centigramme is equal to about five-sevenths of an American grain (apothecaries' weight); consequently, as the smallest dose recommended by the author is five centigrammes, this is five-sevenths of a grain, or we can estimate it as a very light grain.

R. W. T.

The method of administering mercury by the mouth, which I shall minutely describe, is certainly less active than the inunction-method, and it certainly is less rapidly active. For this reason it is preferable as a standard treatment to commence with, as it is less complicated, more convenient, and is generally better borne by patients. This is the treatment which I advise to be always used at the outset.

But there is a more important and practical question in the treatment of syphilis by mercury than the choice of the method or the regulation of the dose, and that is the question of the *length of time* of the treatment.

How long should a syphilitic patient be subjected to a mercurial treatment in order that we may obtain the general action of the drug, and its future beneficial effects, which should certainly be the object of our efforts?

The case may be stated as follows: Suppose a syphilitic patient for whom, in consequence of various lesions, we have prescribed mercury, and that these lesions have disappeared. What shall we do then? Is it necessary to continue or discontinue the mercury? And if it is necessary to continue its use, how shall we do it, and for what length of time? These are eminently practical questions, which deserve a thorough consideration, for upon their solution the future of this patient depends. In the text-books you will find various opinions. For instance, Dupuytren advised that mercury should be administered for the same length of time after the cicatrization of the chancre as was necessary for the cure of the chancre. Other observers have formulated fixed doses which it is necessary to reach in order to cure syphilis; thus eighty to one hundred teaspoonfuls of the liquor of Van Swieten, according to Brousonnet, while Vidal thought that from one hundred to one hundred and ten of Dupuytren's pills were sufficient to exterminate the virulent principle from the organism. According to my views, Chomel was more enlightened, for he attached less importance to the dose administered than to the total duration and continuance of the treatment. He imposed a mercurial treatment of from five to six months as a condition indispensable to a cure, and for probable future safety of the patient. Finally, M. Ricord, in a work[1] which I had the honor of editing for him, has given the results of his long experience in the following words: "*Six months of mercurial treatment* with a daily dose which has an influence upon the manifestations, and which shows after these have disappeared that the medicine still produces its physiological effects; then *three months of treatment by the iodide of potassium*, by which it is intended to prevent the very late lesions of the diathesis. Such is the mode of treatment which gives the most lasting cures, and which, in the great majority of cases, succeeds in really neutralizing the virus, and I would almost willingly say in curing syphilis, at least of curing the greater number of its manifestations."

Now, in my opinion, it is these absolute prescriptions and these *quasi* mathematical formulæ which have done the most injury to mercury; for if, while reposing our confidence in one or other of the methods, we meet with failures, we almost certainly find fault with the remedy and not with the method, and we conclude that mercury is comparatively powerless.

For my own part, I take a wholly different view as to the treatment of syphilis by mercury, and I have been led by force of circumstances, after having followed the methods of my predecessors and of my master, to administer it in a different manner. After having experienced numerous disappointments with the usual methods, I have been forced to introduce several modifications, which, without changing the remedy, give it greater scope of action and obtain better results from it. You and my colleagues will judge as to whether I have succeeded.

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[1] Leçons sur le Chancre. Paris, 1860.

In the first place, I am fully satisfied of the truth expressed by Chomel, that the duration of the treatment is more important than large doses. It is a hundred times better to treat a patient for a long time with sufficient doses of mercury than to give him within a short time large doses. This point, however, is scarcely open to dispute, for it is certain that in order to derive all the good which mercury can give, and to avail ourselves of its curative influence *for the future*, it is necessary to administer it for a longer time than is generally laid down. I have learned the following facts to my cost, by a number of failures:

1. That, if mercury is given according to Dupuytren's method, it does not have a radical effect, but leaves the disease unaffected, and liable to produce all its ulterior consequences.

2. If mercury is given after the manner of Broussonnet, Vidal, and others, we obtain only a half result, or in fact no result, for with this short and settled treatment we only dissipate the present lesions and delay the later ones, and we do not act in a sufficiently powerful manner upon the diathesis.

If you consult the records of observations you will find that a great number of patients treated in this manner for two or three months have been the victims later on of serious and fatal lesions, and I am positive I can say that it is patients of this class who furnish the greater number of cases of tertiary syphilis, because the patients who have been treated by the purely expectant plan are very rare. The following is the usual story of a case of tertiary syphilis: A patient becomes syphilitic, and is treated early in the disease for a few weeks or for two, three, or four months, when all manifestations disappear. He then thinks himself cured, and does nothing more, and he consoles himself with a visionary guarantee of an immunity, when suddenly new lesions develop, sometimes serious, other times very serious, as they perhaps do not have the physiognomy of syphilitic, but resemble non-syphilitic lesions, and these are liable to be mistaken as to their nature, and to be treated in an inappropriate way which tends to future trouble.

3. What I have also seen (less often, it is true) is that, after long courses of treatment, for example, after the one formulated by my illustrious master, whose pardon I humbly ask for this necessary criticism, is that the diathesis manifests itself by later outbursts, thus showing its persistence in the organism. I have in my note-book numbers of cases of patients who, after having been regularly and uninterruptedly treated by mercury for five or six months, have later on developed more or less serious manifestations. It is then certain that a mercurial course, assiduously pushed for five or six months, is by no means always sufficient to extinguish the diathesis or prevent future peril. I make this positive statement, in spite of the respect due to my master, because clinical experience has several times demonstrated it to me. Another fact which I have observed, and of which I am now certain is, that mercury, when given long and continuously, perceptibly loses it efficacy. It is the same with mercury as with other remedies, its continuous use induces a condition of tolerance which lessens and finally destroys its therapeutical effects. In support of this view, I will cite several classes of proof:

1. *Analogical Proof.*—It is an evident and indisputable fact that persons become accustomed to certain medicines, which, having at first been active in their effects, in time have no influence upon them, as the organism is surfeited. For instance, if you take an opium-pill at night, you will experience its effects; if you take one for a number of nights, you will scarcely experience any effect; and if you continue its use for a number of months, you may be certain that it will not exert the least narcotic or sedative action. Now, it is the same with mercury, to which the economy becomes accustomed, and which after a time loses all its influence.

2. *Direct Proof.*—The following fact is often observed: A patient has a syphilitic lesion, upon which a certain daily

dose of mercury has a perceptible effect. The remedy is continued, and then it is noticed that its therapeutic action diminishes or ceases, and the lesion persists as if nothing had been done. In this state of affairs the dose is increased, we will say doubled, when suddenly the therapeutic action is reëstablished, and the lesion begins anew to disappear. Now, what interpretation more simple or rational can be given to the fact, which every observer has seen many times, than that a certain dose of mercury, which, having exerted an influence on the disease for a certain time, beyond that has lost its influence, because the organism has become *habituated to it?*

There is another fact which points in the same direction: It often happens, in the course of a long-continued mercurial treatment, that new lesions show themselves. What happens then, in view of these lesions, if the treatment is continued? Under these circumstances, mercury produces slow and unsatisfactory results, in some cases none at all, and even in spite of accumulated doses of mercury the lesions persist. On the contrary, in a similar case, if we suspend all treatment, and wait without doing any thing for two, three, or four weeks, and then renew the same medication in the same form and dose, we shall see that it acts suddenly as if by magic, as the economy has become again unaccustomed to it, its primitive energy and habitual effects having been temporarily lessened or suspended in consequence of the system having become accustomed to it.

Is it not evident from this that a much-prolonged mercurial treatment does lose a great part of its influence? I am convinced of it for my part. I believe that, when we have placed a patient under a mercurial course for two or three months, the latest doses are a *pure waste*, as by that time the remedy has become, if not inert, at least less active and powerful than at first, in consequence of the system having become accustomed to it. To be precise, I believe that a continuous six-months' mercurial treatment produces infinitely less results than it would if divided into treatments of six or eight weeks in a duration of from twelve to fifteen months. I have seen patients, who for an entire year have not ceased to take mercury, derive less benefit from these enormous doses than they probably would have if they had taken one-half of the quantity intelligently administered.

There are practical conclusions to be drawn from this fact. Taking into consideration these two greater facts which are derived from personal observation, namely, the necessity, on the one hand, of a mercurial treatment *prolonged for a considerable period*, and, on the other hand, the evident disadvantages of a continuous mercurial treatment, I have been led to adopt for my patients a method of treatment somewhat different from those of my predecessors, and which I will call the *method of successive treatments.* Nothing can be more simple than this method, as it is the application of the principles which have been adduced. In order to understand it, an illustration will serve better than any commentary:

Suppose we have here a patient who demands our advice for a papular syphilide accompanied by other lesions, and that I prescribe daily doses of from five to ten centigrammes of proto-iodide of mercury for him? In three or four weeks the syphilide will, in all probability, have disappeared; we, however, prolong the treatment to two months. After that, what shall I do? After that, *whatever may happen* (bear this well in mind), I would suspend the treatment, being very certain by experience that my patient will have already become accustomed to the mercury, of which continued doses would only have a relatively small effect. I would leave him then without treatment for several weeks—to be more definite, at least a month. That time having elapsed (understand this well also), I would recommence the treatment, whatever might have happened, whether the patient has or has not had new lesions; for, although he had not developed other lesions, he would be none the less syphilitic, nor less liable to the manifestations which it is my desire to prevent. So I should institute the same treatment anew for six weeks or two months.

This being done, a respite of three months would be granted, without any great fear, on the one hand, of any serious lesions developing themselves, and, on the other hand, with great benefit in consequence of the patient becoming unaccustomed to the remedy, which condition is favorable to its future action. After that I would renew the mercury for six, seven, or eight weeks; then I would suspend it for several months; then I would prescribe it again, and thus in succession, always with the precaution of allowing each period of active treatment to be followed by an interposed period of repose, in which the patient becomes *unaccustomed* to the remedy.

In following out this course, I should realize what I desire, that is, *I should preserve the peculiar intensity of action of mercury during the whole period of treatment.* This is one of the objects of my efforts; and, as we shall see, this method of interrupted treatments is none the less favorable to the second.

But, before going any further, I ought particularly to state that the scheme of treatment which I recommend cannot be laid down in a fixed and absolute manner. A treatment, in fact, does not admit of unvarying rules, and cannot be laid out in advance like the course of a chronometer, or of the scenes of a comedy. Like all other methods of treatment, this one is subordinate in its application to the special exigencies of each case. Thus, as the indications arise, the duration of the periods of treatment should be lengthened in one case and shortened in another; then also it will be expedient sometimes to lengthen and sometimes to diminish the duration of the intervals of repose. Thus the succession of different periods can be beneficially modified, according to the intensity of the disease, the tolerance of the patient, the frequence and character of the relapses, the period of the diathesis, and other indications which it is impossible to foresee. This, though very important in practice, cannot be laid down in general prescrip tions. As regards treatment, there is a point to which I ought to call attention, which I have learned by experience: that is, the necessity, at the *début* of treatment, of bringing the periods of treatment close together, and, on the contrary, of lengthening them apart more and more in proportion as we reach the later stage of the disease. Thus, at the commencement, between the second and third periods of treatment at the most you can only intercalate periods of repose of from six to eight weeks; whereas later on you can prolong them, without disadvantage or fear of accidents, to two or three months; *a fortiori*, you will derive benefit in a later stage of the disease by interposing between the active periods a space of from four to five months. This is what has appeared, at least empirically, most beneficial to my patients.

The second intention of this method is to confer upon patients the advantages of a long-continued treatment, and this method is better adapted than any other to this essential indication. In fact, it enables patients to be treated for a long time without wearying them, and to take for as long a period as may be necessary a remedy which, if continuously administered, would not be long either in being not tolerated, or of losing its curative action.

A mercurial course should be prolonged for a long time, if we desire its permanent action. The whole duration of treatment cannot be determined in a fixed and invariable manner; it is subject to the indications of each case to the various circumstances which may arise, to the intensity of the disease, to its form, and to the frequence and character of the relapses. It would be absolutely necessary in one case to insist upon a long treatment, and in another case, owing to its mildness and to the prolonged absence of relapses, to stop medication much sooner. On an average, I can say that it is necessary to keep patients under a mercurial treatment for two years. I do not advocate two years of continuous treatment, but rather two years divided into about ten months of treatment and fourteen months of repose. I do not exaggerate when I say two years, for in the first place there are rebellious cases in which circumstances render it necessary to continue treatment beyond that period, and, in the second place, if we consult general pathology we shall see that the greater number of constitutional diseases or diatheses require a proportionately longer period of medication even than syphilis does. The rule is, that chronic diseases demand a chronic treatment. Do you think that we can cure the gout by a few weeks' stay at Vichy, or by a few months of treatment, or that scrofula can be cured by a stay of a few weeks at the sea-shore, or by a treatment of some months with cod-liver oil and the preparations of iodine? It requires several years to modify these diatheses, and the same is true of the syphilitic diathesis, which is not modified or effaced except at the expense of a long medication, a truly *chronic* treatment. Therefore, I think I am not guilty of exaggeration in fixing a medium duration of two years for the period in which syphilitics should be treated by mercurials in the manner I have explained.

Still this is not all, for I am one of those who are of the opinion that iodine should be combined with the mercurial treatment later on, as I think that iodide of potassium is very beneficial to syphilitics at one stage of their disease. I think that the iodide is indicated toward the end of the first year, and during the course of the second, and I think it is indispensable in the third year, not then combined with mercury nor used alternately with it, but used alone as a preservative against tertiary manifestations. I will not, however, touch the treatment of the tertiary stage, as I will reserve that for our meetings next year, and will only state the advantages of a treatment of iodide of potassium, following a mercurial course. These are my views of an antisyphilitic treatment, and this is the manner in which it should be instituted and carried out. I would say that I have not adopted this method from theory; I have arrived at it by force of circumstances, by experimenting, and in consequence of the evident failures of the other treatments.

Capable, undoubtedly, of being rendered more perfect, this method offers important advantages which cannot be denied to it. Being superior to the continuous treatment, it is acceptable to patients, is readily borne by the organism, and preserves the integrity of the mercurial action during the whole period of its administration, and can be thus administered during the whole time necessary to the cure.

Undoubtedly it is not infallible, as there are some cases rebellious to it. I can, however, say that I have in general derived satisfactory results, especially when compared with the results of other modes of treatment.

I have thus treated thousands of patients during twelve years, and, with few exceptions among the great number whom I have been able to see again and to follow, their future health has not been impaired by the diathesis; many among them have been married and have had healthy children. I do not know, and I will not say, that they are absolutely cured, but the treatment has rendered their syphilis slight in the past, latent for the present, and in all probability little to be feared in the future.

I have now considered the *specific treatment* of syphilis, and it remains for me to consider its general treatment.

It is not sufficient in the treatment of syphilis to merely administer mercury or the iodide of potassium, but it is necessary to look after the health of the patient, to watch his temperament, his constitution, and his strength, and the various contingencies which might arise, and to meet all these indications, and from these to formulate and combine auxiliary treatment with the specific.

In women especially, auxiliary medication is often of great importance, so much so as to become the principal treatment by superseding the specific treatment.

Syphilis of women[1] differs from that of man, especially in the fact that it affects the organism more profoundly. In women, oftener than in men, the functions of digestion, circulation, assimilation, nutrition, and innervation, are affected by syphilis, and she is also more liable to the secondary visceral lesions of syphilis. In her we frequently see syphilis produce anæmia, asthenia, loss of strength and flesh, deterioration of the constitution, and sometimes sufficiently grave disturbances as to compromise life. Undoubtedly syphilis does not assume these alarming characters with every woman, but they are more frequently observed in the feminine than in the masculine sex. Therefore, the health of the syphilitic woman demands especial watchfulness and attention from the physician, and gives rise to indications beyond the specific treatment which are imperative to meet.

I cannot insist too much upon this advice; when you treat a syphilitic woman do not confine yourself to treating her syphilis; *do not believe you have done every thing when you have administered mercury*, for there is something else to be done. Look upon her as a sick person, examine all her functions, have an eye to her general condition—in a word, be watchful of her health.

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[1] It will be noticed that throughout this article particular mention is made of the course and treatment of syphilis in women; the reason of this is, that at the Lourcine Hospital none but women are treated, consequently, they are the subjects of the clinical lectures delivered there. While these observations by M. Fournier are very suggestive as to the course, and to the modes of treating syphilis in women, the directions for treatment are none the less valuable, as being applicable to men, if it is only remembered that the latter require a somewhat larger dose than the former. It is also necessary to remember that syphilis, in many men, affects the organism as profoundly as it does in women.—R. W. T.

To this end it will be necessary at first to pay attention to the patients' hygiene, to inform yourself of the details of their mode of life, their regimen, their occupation, etc.; it will be necessary to insist upon the necessity of a calm and regular life, to advise for them a nutritive diet, in which meat and wine enter largely, a daily amount of exercise and sufficient sleep. To this end also it will be necessary to prescribe tonics, namely, in the first place iron, which is beneficial in the chloro-anæmia and asthenia so often observed in the secondary period of syphilis in women; quinine, and the bitter tonics; cod-liver oil; saline and sulphur baths, sea-bathing, shower-baths, and hydro-therapeutics, which have been known to produce excellent results; a residence at the sea-side or in the country, etc. These tonic agents are in many cases indispensable auxiliaries to the specific treatment, and often contribute as much as it to the success of our efforts.

Finally, there is a last word of advice:

This long therapeutic programme having been punctually followed, how shall we reply to a patient who, at his last visit, puts the following question to you, which you may be sure he will do: "Now, doctor, am I finally rid of this disease? Do you think that I am radically cured?"

To this you must reply as you think, and as science warrants you to think and to hope. Now, what you really think is that your patient has every chance of being free from syphilis now and forever. You are, in a moral point of view, warranted in saying this.

But you also think that, in spite of your long and active course of treatment, this patient might be exposed at some future day to new and later manifestations. For, unfortunately, there is no sign which permits us to *affirm a cure in syphilis*, for it is, as M. Ricord says, "neither the dose, nor the pharmaceutic preparation, nor the duration of the treatment, which confers an absolute immunity, or which can guarantee the radical extinction of syphilis." It is necessary to say this to your patients.

We may promise health or a cure to a person dying either of phthisis or cancer perhaps, for that is a pious lie, and is a consolation which, in our helplessness, we owe to our unhappy patient, and which it would be cruel to refuse; but to a subject in full health, who enjoys all his faculties, and who, moreover, we are almost warranted in believing to be forever rid of his disease, we owe him the truth and that only, and it is for his benefit that he should know the truth.

Should a syphilitic manifestation develop upon such a patient, say ten, fifteen, thirty, or forty years after contagion, it is possible that it would not call to his mind the remembrance of a disease, a long time vanished and almost forgotten, and that the physician, not being aware of his syphilitic history, might not recognize the syphilitic character of the lesions, more especially as these later lesions are far from presenting a typical syphilitic appearance. This may be a lesion of a viscus, a tumor in the brain, a sclerosis of the cord, an amaurosis, or it may be an hemiplegia or a paraplegia. Now, in the eyes of a layman, what connection could be associated between this manifestation and an old sin of youth past and gone and expiated years ago? What is the necessity of making a full confession to the physician, and of reviving such compromising reminiscences? The result would be that the patient would say nothing of his previous syphilitic history, and the physician not being informed, would run the risk of mistaking the nature of the trouble. In this case, the lesion not being understood, it is not treated with the only remedy which is beneficial for it, therefore it persists and follows its normal evolution, and may result in a grave or fatal termination, when otherwise it might have had every chance of being cured if it had been traced to its true origin and treated by its specific. You may be certain of the fact that a number of persons having tertiary syphilis have become afflicted with incurable infirmities, or even have died simply from the fact that a late syphilitic lesion has not been recognized and treated as it should have been. Now, it is necessary to warn your patients against this possible contingency. The possibility of tertiary lesions manifesting themselves in the future makes it of importance that our patients should be fully and clearly made aware of their true state when we dismiss them as cured.

Never forget this when one of your patients, at his last visit, shall put this serious question to you: "Am I cured?" Do not neglect to tell him what you think, and, as an adieu, give him this salutary but very essential advice:

"Yes, I think you are cured. I think science will warrant me in saying that. But whatever may happen to you in the future, whatever disturbance you may have to your health, *remember your old disease.* Tell your physician of it. Do not at any hazard fail to acquaint him of your syphilis. Tell him plainly, ten times rather than once, that you have had syphilis in earlier days. It is probable that this information may be of no service to him; but it may happen that it is very important for him and for you especially, for upon it your cure and perhaps your life may depend."

Such, then, is my method of treatment, and such are the conclusions to which my observations have led me. It is a matter of regret to me, in concluding this portion of the subject (for I propose to describe the treatment of tertiary syphilis next year), that I am unable to state these views upon greater authority; but what I have seen I have scrupulously and carefully observed, and it remains for you and for future observers to determine absolutely whether I have carefully observed what I have seen, and whether I have drawn true therapeutic conclusions.

ART. III.—*Quantitative Analysis of Urea.* By GEORGE B. FOWLER, M. D., New York.

IT very frequently becomes interesting and important to determine the amount of urea eliminated by individuals under treatment for various disorders. Generally the medical attendant contents himself with a chemical and microscopical examination of the urine, neglecting the equally important matter, the quantity of urea.

This last I am persuaded is the case, because the methods generally recommended for the quantitative analysis of urea are capable of being understood and practised only by professed chemists, and require for their performance reagents and apparatus rarely seen outside the laboratory.

In 1854, Dr. Davey, of England, published in the *Philosophical Magazine* the description of a method devised by himself for estimating the quantity of urea in a given solution.

This analysis depends upon the decomposition which occurs from the union of the hypochlorites of soda, potash, or lime, with urea, where the urea gives off its nitrogen, which being collected is measured, and the quantity of urea originally present estimated.

The simplicity of this method at once recommended it to general favor; but a question immediately arose as to its accuracy, to which is due the fact of its being little known and practised. I therefore thought it worth while to endeavor to ascertain if this easy method does yield reliable results.

The following are Dr. Davey's directioned: "A strong glass tube, about twelve or fourteen inches long, closed at one end, and its open extremity ground smooth, and having the bore not larger than the thumb can conveniently cover, holding from two to three cubic inches, each divided into tenths and hundredths by graduations on the glass, is filled more than a third full of mercury, to which, afterward, a measured quantity of urine to be examined is added, which may be from a quarter of a drachm to a drachm or upward, according to the capacity of the tube; then holding the tube in one hand near its extremity, and having the thumb in readiness to cover the aperture, the operator fills it completely full with a solution of the hypochlorite of soda, taking care not to overflow the tube, and then instantly covers the opening tightly with the thumb, and, having rapidly inverted the tube once or twice to mix the urine with the hypochlorite, he finally opens the tube under a saturated solution of common salt and water contained in a steady cup or mortar. The mercury then flows out, and the solution of salt takes its place, and the mixture of urine and hypochlorite, being lighter than the solution of salt, will remain in the upper part of the tube, and will therefore be prevented from descending and mixing with the fluid in the cup. A rapid disengagement of minute globules of gas soon takes place in the mixture in the upper part of the tube, and the gas is collected and retained. The tube is then left in the upright position until there is no further appearance of gas being formed, the time being dependent on the strength of the hypochlorite and the quantity of urea present, but the decomposition is usually completed in from three to four hours; it may, however, be left much longer, even for a day, if convenient, and, having set the experiment going, it requires no further attention. When the decomposition is completed, it is only necessary to read the quantity of gas produced off the scale on the tube. In cases where great accuracy is required, due attention must be paid to the temperature and atmospheric pressure, and certain corrections made if these should deviate from the usual standards of comparison, at the time of reading off the volume of gas; but in most cases sufficiently near approximations to accuracy may be obtained without reference to these particulars."

Dr. Davey recommends the use of the hypochlorite of soda, as it is a preparation easily obtained under the name of liquor sodæ chlorinatæ, or Labarraque's solution, and it moreover does not soil the sides of the tube as do the others. Dr. Flint, Jr., in his book on the "Chemical Examination of the Urine," speaks particularly of the importance of employing the French Labarraque, as the American is unreliable. When I came to inquire, several preparations of the American Labarraque were presented to me, which in the experiment yielded different results. I found also a number of French or imported articles which too were unlike in quality. It therefore became of the first importance to select a preparation of the hypochlorite of soda of the most reliable manufacture, one the strength or quality of which would not vary, and one generally kept by chemists. Squibb's liquor sodæ chlorinatæ answered my purpose so far as the above requirements were concerned, and it only remained to test its value in connection with Davey's method. I procured a cubic-inch tube and made several analyses. In each of them I failed to collect, according to the graduations on the tube, sufficient gas to correspond with the known quantity of urea introduced. But, observing that the same amount of gas was present in every case, the urea being the same, I was led to doubt the accuracy of the tube, and upon examination it proved to be inaccurate, its capacity being greater than the graduations indicated. Finding it impossible to obtain one in the city of any different manufacture, I resorted to a cubic-centimetre tube of French make which was correctly measured and marked. With a cubic-centimetre tube, therefore, the following experiments were made—in each instance, however, the results in cu. cent. were converted into cu. in. simply by multiplying the number of cu. cent. of gas obtained by .06103—, because one cu. cent. = .06103 cu. inches.

By calculation the fifth part of a grain of urea should furnish .3098 parts of a cubic inch of gas at 60° Fahr. and 30 bar. Dr. Davey employed one-fifth of a grain in two experiments; in one he obtained .3001, and in another .3069 cubic inches, the variation being only in the thousandths of a cubic inch. The final result is obtained by the process of the Rule of Three. A grain of urea should furnish 1.549 cubic inches of gas, then 1.549 : vol. gas found :: 1 : answer. In short, divide the volume of gas found by 1.549, and the answer will be the amount of urea in grains.

Experiment I.—I made a solution of urea, gr. ij to 10 cubic centimetres of water. Of this solution I took 1 cubic centimetre, which contained one-fifth of a grain of urea. Having filled the tube a little more than a third full of mercury, I added the cubic centimetre of the solution of urea. The Labarraque solution was then poured in until the tube was full. Then instantly closing it with the thumb, and rapidly inverting it several times, I opened it under a saturated solution of salt and water. The gas immediately showed itself in the top, and accumulated with the falling rapidity—the temperature being 65° Fahr., barometer 29.

In ½ hour there were 3 cubic centimetres of gas evolved.
" 1 " " " 3⅗ " " " "
" 2 hours " " 4 " " " "
" 4 " " " 4⅕ " " " "
" 5 " " " 5 " " " "
" 24 " " " 5 " " " "

As 1 cubic centimetre equals .06103 cubic inches, 5 cubic centimetres will be .30515 cubic inches. And now to ascertain the number of grains in the solution of urea tested. A grain of urea should furnish 1.549 cubic inches of gas—there will be as many grains in .30515 cubic inches of gas as 1.549 is contained in it, which is .19+ grain.

EXPERIMENT II.—Solution gr. ij.—10 cubic centimetres water. Used 1 cubic centimetre = ⅕ gr. urea. Thermometer 90° Fahr. Barometer 29.

In	½ hour	there were	3⅕	cubic centimetres	of gas evolved.
"	1 "	" "	8⅘	" "	" "
"	2 hours	" "	4	" "	" "
"	4 "	" "	4⅘	" "	" "
"	5 "	" "	5	" "	" "
"	24 "	" "	5	" "	" "

.06103 × 5=.30515 cubic inches of gas.

30515÷1.549=.19+ grains of urea.

EXPERIMENT III.—Solution of urea gr. ij.—10 cubic centimetres water. Used 1 cubic centimetre = ⅕ grain of urea. Thermometer 68° Fahr. Barometer 30.

In	½ hour	there were	4⅗	cubic centimetres	of gas evolved.
"	1 "	" "	4⅗	" "	" "
"	2 hours	" "	4⅘	" "	" "
"	4 "	" "	5	" "	" "
"	5 "	" "	5	" "	" "
"	24 "	" "	5$\frac{1}{10}$	" "	" "
"	48 "	" "	5⅕	" "	" "

5⅕ cubic centimetres = .31735 cubic inches. .31735 cubic inches ÷ 1.549 cubic inches = .204 grains of urea.

EXPERIMENT IV.—Solution of urea gr. ij.—10 cubic centimetres water. Used 1 cubic centimetre = ⅕ grain of urea. Thermometer 85° Fahr. Barometer 28.

In	½ hour	there were	8	cubic centimetres	of gas evolved.
"	1 "	" "	8⅗	" "	" "
"	2 hours	" "	4⅕	" "	" "
"	4 "	" "	4⅘	" "	" "
"	5 "	" "	5	" "	" "
"	24 "	" "	5	" "	" "

5 cubic centimetres = .30515 cubic inches of gas.

.30515 ÷ 1.549 = .19+ grains of urea.

In these four experiments, then, the results are strikingly accurate, and correspond very closely to those of Dr. Davey; and, what is the most important, do not differ except in a few thousandths of a cubic inch from the result which the same quantity of urea should give by calculation.

By calculation, a grain of urea should furnish	Dr. Davey's results.	My results.
.3098 cubic inches of gas.	.3001 cubic inches.	.30515 cubic inches.
	.3069 " "	.30515 " "
		.31735 " "
		.30515 " "

In Experiment III. it will be seen that the gas was more rapidly evolved from the first, and after five hours increased one-fifth of a cubic centimetre. The variations of the barometer and thermometer were not sufficient to cause it, and I rather think it due to a slight inaccuracy in measuring the quantity of urea solution introduced.

I varied these experiments by adding less and more urea, and in each instance the number of cubic inches of gas collected was in proportion to the quantity of urea present.

I think, therefore, I am warranted in recommending this method to the profession. It is sufficiently accurate for all practical purposes, and requires but little time and apparatus for its performance. I would urge the use of Squibb's liquor sodæ chlorinatæ, and advise care in selecting the experimental tube.

Clinical Records from Private and Hospital Practice.

I.—*Case of Shark-bite.* By J. Fayrer, M. D., Calcutta. [Medical Times and Gazette, June 15, 1872.]

Deno, a muscular, healthy Oorya coolie, aged thirty, was admitted into the Medical College Hospital, on March 26, 1871, two days after having been severely bitten by a shark while bathing in the Mutlah. He had been rescued by his companions, but not before the savage creature had seized him three different times, inflicting most serious injury, tearing away a large portion of the left thigh, and causing great effusion of blood. There was an enormous wound extending from about the middle of the left gluteal region to within three or four inches of the ham. It measured twenty inches in length, seven in breadth at the lower end, twelve inches in the gluteal region, and nine inches at its upper extremity. The depth was great, extending nearly to the bone, which could be felt only covered by a few muscular fibres. The flexors and great part of the glutæus maximus were torn away; the sciatic nerve divided, and several inches of it removed. The anterior aspect of the limb had escaped, and the femoral vessels and nerve were uninjured. He was depressed and feeble from shock, loss of blood, and pain; his pulse 112—better than might have been expected. He was restless and feverish. He appeared to have been a healthy and vigorous man before the accident. Notwithstanding the injury, he was able to move the limb slightly; he could approximate the knees and flex the limb, showing that some portions of the flexors were undivided. Sensibility seemed to be preserved in the integument of the leg and foot. In addition to the wound above described there were others—one on the right gluteal region six inches in length by four in breadth, the integument to this extent having been torn away and the subjacent tissues deeply lacerated; another on the right elbow, which had taken away a narrow strip of integument about two inches in length. There were also several deep punctures in the back, corresponding to the size of the shark's triangular teeth. These wounds were all superficially sloughing when he was admitted.

In consultation with my colleagues, it was determined to attempt to preserve the limb. The loss of so much muscle, the division of the sciatic nerve, and the great extent of the wound, were suggestive of immediate amputation at the hip-joint; but as the joint itself was uninjured, and his condition as favorable as could be expected under such circumstances—considering the shock and loss of blood (which is said by his companions to have been very great)—it was determined to attempt to save him without the amputation at the hip, itself a most dangerous alternative. His bowels being confined, a dose of oil was ordered, and acetate of ammonia with nitric and chloric ether prescribed with reference to his feverish condition.

Opium was freely administered, to allay pain and give rest. The limb was supported on a splint, and the wound dressed with poultices and carbolic-oil dressing. Subsequently decoct. cinchonæ with quinine were given, and chloric ether. He was slightly delirious on April 2d. The pulse increased to 120. Temperature varied from 97.5° in the morning to 101° to 103° in the evening. But the wounds had cleaned and presented a healthy granulating surface on April 3d, covered with well-formed pus. He was very restless at night, and could not sleep, complaining of burning in the wounds. Hydrate of chloral was given, but with little benefit. An abscess formed on the left shoulder about this time (April 9th), which soon degenerated into a sloughing sore, from which a slough of subcutaneous cellular tissue separated. The wrist and forearm became œdematous. From the 10th he again began to improve, and slept better. The discharge moderate; the wound looking pale and pink, with imperfect granulations, but evidently contracting. His pulse fell from 120 to 110, and the temperature from 103° to 101° to 100°. A slight burrowing of matter with sloughing of subcutaneous cellular tissue occurred; this being laid open on the 18th, the wound soon began to granulate, and he continued to improve. On the 25th the surface of the wound had contracted three inches in length and one inch in breadth on the thigh, and two inches in the loins. On May 3d he had slight diarrhœa, which was checked by astringents. He now began to look more pallid and puffy about the face and limbs, but the urine contained neither albumen nor sugar. The wounds, though inactive, continued to contract. Potassio-tartrate of iron and infusion of calumba were prescribed, with two grains of opium every fourth hour to allay irritability and promote capillary action. The puffiness diminished for a few days, but it again returned, and the pulse became weaker and more rapid, and he again began to complain of a burning sensation in the wound; sleep was imperfect, and fever and cough supervened on May 16th, which continuing for fifty-six hours, left him very weak. He never rallied from this condition, and he gradually sank on May 21st.

On *post-mortem* examination the left lung was found to be congested throughout, the lower lobe being in a state of gray hepatization, pus freely exuding from its cut surface. The right pleural cavity contained a quantity of sero-purulent fluid. The pericardium contained about four ounces of serum, and there was a small fibrinous clot in the right ventricle. The liver and kidneys were fatty, and the latter congested. The whole muscular system was pale and flabby. The wound was cicatrizing throughout; that on the back had healed. On dissection it was found that about seven inches of the great sciatic nerve had been torn away. The muscles on the back of the thigh were also torn away. The semi-tendinosus and membranosus, the long head of the biceps, and the gracilis, had suffered; their divided ends were found agglutinated together with the lower end of the sciatic about three or four inches above the ham. The surface of the sore was formed by the short head of the biceps, vastus externus, and outer surface of the trochanter major; the edge of the sartorius with the adductors on the inner side. In the gluteal region it was formed by the surface of the external rotator muscles of the thigh, the pectinæus and quadratus femoris below, the deep fibre of the glutæus maximus above. The gracilis was torn across at about an inch from its origin, and the upper end of the divided great sciatic nerve was on a level with the edge of the lower margin of the quadratus femoris, upon which it lay thickened and adherent. The end of the nerve was bulbous, and tied down by the cicatricial tissue that matted all together. The general condition of the patient and the reparative action in the wound all at one time seemed to promise a favorable termination of the case, but pyæmic mischief, resulting in suppurative pneumonia of the left lung, supervening in the seventh week, proved rapidly fatal.

II.—*Cæsarean Operation; Recovery.* By PHILIP FOSTER, M. D. [Lancet, June 1, 1872.]

MRS. H. G., a primipara, aged twenty-two, sent for me at 5 A.M. on the 17th March last, to attend her in her confinement. She had had pains at long intervals all night. Upon examination, the pelvis was found to be much deformed, and the os uteri out of reach. As the pains were very slight and infrequent, the patient, after recommending her to keep as quiet as possible, was left for the present. She was seen at noon, and again at 10 P. M.; when, the pains still remaining trifling, an opiate was prescribed, and she was left for the night.

March 18th.—10 A. M.: On calling this morning I learned that she had passed a very good night, having been disturbed very little by the pains. I saw her again at 4 P. M., and made a careful examination of the pelvis: the extent of its conjugate diameter was barely an inch, and its lateral not more than two inches. The os uteri was still out of reach. At a consultation now held with my father and brother, it was decided that it was utterly impossible to deliver *per vias naturales*, and that nothing remained to give the patient a chance of life but the Cæsarean section. The peculiarities of the case and the extreme danger of the operation were fully explained to the patient and her friends, who requested us to do whatever we might consider best. I should observe that the liquor amnii had been escaping for two days, and that there was reason to believe the child dead, as it had not been felt to move for twenty-four hours.

Operation.—Assisted by my father and brother, I commenced the operation at 6 P. M. by an incision through the abdominal parietes in the usual direction. It was six or seven inches in length, and extended from about half an inch above and a little to the left of the umbilicus nearly down to the pubes. This was followed by a corresponding incision, about five inches long, through the walls of the uterus. In laying open the abdomen, the index and middle fingers of the left hand, as soon as the cavity was reached, were introduced and used as a director, and carried, one on each side, a little in advance of the knife while the incision was being prolonged. The child dead, as was anticipated, was now removed and separated, and, the uterus immediately contracting firmly, the placenta was expelled through the wound, and required no effort to extract it. The hæmorrhage was very slight—not more than seven or eight ounces. After sponging away the blood from the abdominal cavity, the wound was closed by long stitches of the uninterrupted suture, care being taken not to include the peritonæum. The ligature used was stout silk. Broad strips of adhesive plaster were then applied crosswise, and over these were placed a thin flannel compress and broad binder. The intestines protruded through the upper part of the wound, and great care was necessary to avoid injuring them. My brother, by gentle manual pressure, kept them nicely out of the way. The whole proceeding occupied just a quarter of an hour, five minutes sufficing for making the incisions and extracting the child and placenta, the rest of the time being taken up in closing the external wound. The patient went through the ordeal well, only two or three half-suppressed moans escaping her during the operation. At its conclusion she expressed herself as feeling pretty well. Chloroform was not given, as it was thought that it might have a depressing effect, and interfere, perhaps, with the contraction of the uterus. She was ordered two grains of opium immediately, and not to take any thing except a little tea, milk, with brandy-and-water at discretion. At 10 P. M. the patient said she felt nicely, and had very little pain. Tongue clean and moist; but the pulse rather quick, 100; countenance good, and the temperature of the body not increased. The abdominal wound did not heal by first intention, but the patient was well and able to do light work by May 9th. Dr. Foster says, in comment on the case: "The successful result was owing, in the first place, to the *early performance* of the operation, before the strength of the patient had become impaired by the long continuance of useless pains; secondly, to the free use of opium, which I am convinced was of essential service, by tranquillizing the nervous system, checking inflammatory action, and procuring sleep; and also to the timely resort to stimulants and sustaining treatment."

Bibliographical and Literary Notes.

ART. I.—*The Physiology of Man. Nervous System.* Vol. IV. By AUSTIN FLINT, Jr. D. Appleton & Co., 1872.

THIS long-expected volume of Dr. Flint's will be welcomed by the large class of readers to whom, owing to the ever-growing importance of nervous affections, a suitable guide to the anatomical arrangement and physiological functions of the nervous system has become a necessity. To these the work will bring no disappointment. The style is admirable, the matter well arranged, and the vast literature of the subject has been so thoroughly winnowed that the reader may feel confident that in these four hundred and sixty pages nothing but the chaff will be found wanting.

As the volume is itself a critical digest of the subject which it treats, by one of the most accomplished of living physiologists, the task of the reviewer is confined to the calling of attention to some of its characteristic features and doctrines.

The author, influenced, doubtless, by his determination to furnish mainly "statements of facts that will probably not undergo serious modification, as we advance in our knowledge of the subject," will be found to have treated with coolness a good many of the novelties and speculations that have been zealously put forth during the past few years. An example of this we find in the opening chapter on the anatomy of the nervous system. As an anatomical fact, the axis-cylinder of many nerve-fibres possesses longitudinal striations. These, according to Schultze (Stricker's "Handbuch"), are produced by fibrillæ. One of the modes of termination of nerves consists, according to the same authority, in the separation of these bundles of fibrillæ and their distribution to special cell-elements (organs of special sense, epithelium of glands, etc.). Again, when we come to study the origin of nerves, these fibrillæ are found to pass, not into the nerve-cells, but through them, by means of the prolongation of the cells, into the adjacent tissues. The nerve-cells become, then, not the starting-point of nervous influence, but simply the starting-point of the axis-cylinder pole, each cell furnishing but a single pole. The real source of nervous influence would have to be traced back through the nerve-cell prolongation to some source as yet undiscovered. This would give us, then, the following origin, course, and distribution of nerve-fibres: they begin as bundles of fibrillæ which converge to form the nerve-cells, then pass out by a single pole, constituting the axis-cylinder; and finally, on arriving at the point of destination, split up into the primitive fibrillæ, which severally are distributed to their respective destinations. The cardinal point of this doctrine is the fibrillation of the cells and their prolongations. Now, our author, while admitting the striations in some of the lower orders of animals, regards the existence of a similar arrangement in the human subject as still doubtful, and cautiously adds: "While this question is so important that it can hardly be neglected in studying the physiological anatomy of the nerve-centres, it is one concerning which it is impossible at present to express a positive and definite opinion" (p. 50). He naturally regards, therefore, "the cells of the gray matter and the axis-cylinder of the nerves as probably the only anatomical elements concerned in innervation," though it may be still proved, if Schultze's views should be demonstrated as correct, that, in the final result, the accessory anatomical elements in the nerve-centres are not altogether destitute of influence.

The terminations of nerve-fibres in the nucleoli of muscular fibre-cells, according to Frankenhaeuser and Arnold, and in the cells of the salivary glands, as described by Pflüger, find approving mention in the text; though attention is duly invited in the foot-notes to the element of uncertainty contained in these recent researches.

In the chapter on the motor and sensory nerves we have presented the controversial evidence in favor of Magendie's claim to the discovery of their functions, the facts of which had already been given by the author, in a paper entitled "Historical Considerations concerning the Properties of the Roots of the Spinal Nerves," published in the QUARTERLY JOURNAL OF PSYCHOLOGICAL MEDICINE in 1868. The need of giving to this evidence a permanent form will be apparent by reference to a paper on the "Letters and Discoveries of Sir Charles Bell," which has lately appeared in the *Edinburgh Review*, April, 1872. In this paper the Scotch Podsnap waves aside all the difficulties in the way of the claims of his favorite, in the following majestic manner: "His (Bell's) *brochure* of 1811 enabled him ten years afterward to scare away pilferers from his reputation." And, farther on: "But, sure as vultures to the prey, came swooping down detraction and envy to rob him of his rightful honors. Those of the school of Magendie, vivisectors, who maintained that no system of investigation could be trustworthy which was not founded on actual experiment, claimed to have obtained similar results from their operations, before Sir Charles Bell published his views. A controversy on the subject of prior discovery lasted for some years, but time and universal opinion have now settled it; and Sir Charles Bell's claim is acknowledged by all." To be sure, the writer does disingenuously state subsequently that it is not quite certain whether the views of Magendie and Bell are precisely similar, but insists that the anatomical deductions of Bell are more likely to be correct than the misleading results of vivisection. Here the writer states what, if he be a scientific man, he knows to be false, as nothing is more dangerous than anatomical deductions, and besides makes an unworthy appeal to the prejudices of the British public.

It is, however, an undeniable fact that, up to a very recent date, the credit of the greatest physiological discovery of modern times has been given almost without exception to Bell. When the latter visited Paris, we are told, Roux dismissed his class, after Bell had been introduced, with the words: "C'est assez, messieurs, vous avez vu Charles Bell." This error was partly due, of course, to ignorance of the precise nature of the pamphlet of 1811 (upon which Bell's claims were based), and partly, in all probability, to Magendie's unpopularity with his contemporaries. In Paris it was the custom to term him contemptuously "un grand farceur." His countryman Longet never can mention his discoveries with patience. Hyrtl, of Vienna, speaks of him as "one to whom physiology has so much to pardon." However, it seems now conclusively established, notwithstanding the garbled extracts from his writings furnished by Longet, that Magendie was the actual discoverer of the properties of the roots of the spinal nerves; and Dr. Flint has done well to incorporate the evidences of the fact into his work on physiology.

Under the head of "Rapidity of Nervous Conduction" we have all the recent calculations of Helmholtz, Marey, and others. Those who have been in the habit of considering nerve-force as something equivalent or kindred to electricity, will be surprised to learn that, in the human subject, the rate of nervous conduction is estimated at from one hundred and eleven to two hundred and fifty-four feet per second. At the latter rate it would take over four days to transmit a telegraphic message across the Atlantic; which, perhaps, will give an idea of the extravagance of the popular expression concerning the "lightning-like rapidity of thought."

Electrotonus, anelectrotonus, and cathelectrotonus, are all hard words. Dr. Flint has succeeded, however, in explaining the phenomena for which they stand in a perfectly intelligible manner. The subject does not furnish light reading, though, and we most respectfully decline to consult the "lucid exposition of these laws" by Dr. Rutherford, as advised by our author.

The existence of a galvanic current from the exterior to the cut surface of a nerve excited some years ago a good deal of attention. The discovery of like currents in muscular tissue has deprived the phenomenon of any special physiological importance.

We do not recollect having seen elsewhere all the facts so fully presented concerning the motor oculi communis. The division of this nerve within the cranium is followed, it is well known, by dilatation of the pupil. This dilatation in rabbits is not immediate. For a time the pupil contracts slowly and feebly under the influence of light, though it will dilate under the influence of belladonna, and can be made to contract by operating upon other nerves, as by division of the fifth nerve, or section of the sympathetic. "These facts," we are told, "show that the third nerve is not the only one capable of acting upon the iris, and that it is not the sole avenue for the transmission of reflex influences."

In noticing the decussation of the fibres of the facial in the floor of the fourth ventricle we have given the curious results of Vulpian's experiment, consisting in a longitudinal section in the middle line of the ventricle. When this is done there is no paralysis of the facial nerves upon either side; but the synchronism in the movements of the orbicular muscles is destroyed. Animals wink with either eye alternately, or with one eye at time. Anatomists have not yet succeeded in demonstrating the decussations which necessarily exist as shown by the well-known facts concerning facial paralysis in hemiplegia. When there is a lesion of the brain-substance anterior to the pons Varolii the facial paralysis and hemiplegia are on the same side, and opposite to the side of injury to the brain. When the lesion is in or below the pons, the face is affected on the same side, and not on the side of the hemiplegia. The inference to which we are led is, that the nerves decussate anterior to the pons, even though this has not yet been actually shown with the scalpel.

All are familiar with the fact that galvanization of the pneumogastric produces arrest of the heart's action. All are not, however, so familiar with the experiment of Waller here given, that this effect is not produced after the extirpation of the spinal accessory nerve, to the fibres of which, mingling with those of the pneumogastric, the inhibitory influence is due.

The reason why the eye inflames after division of the fifth pair of nerves has puzzled most physiologists. It was shown by Bernard that, when the section is made through the ganglion of Gasser, fibres of the sympathetic likewise become involved. This accounts for the consecutive hyperæmia, but the same hyperæmia follows division of the sympathetic in the neck, and is unattended by inflammation. An experiment of Prof. Mason, of Buffalo, quoted by our author, throws some light upon the subject. In section of the fifth nerve, the small root (motor) which presides over mastication is likewise divided. The consecutive paralysis of the muscles of mastication renders nutrition difficult. Prof. Mason found, however, in the case of a cat, in which the fifth pair were divided, that, so long as the nutrition was maintained by feeding the animal carefully on milk and finely-chopped meat, inflammation did not take place, though, when the food was diminished to about the quantity it would be able to take without any special care, the usual inflammatory phenomena presented themselves. Dr. Flint concludes, therefore, that, in addition to the hyperæmic condition resulting from division of the sympathetic, and consequent exaggerated nutrition, there is likewise deficient introduction of nutritive matter, from the paralysis of the muscles of mastication, and, from these combined causes, "the nutritive processes in these delicate parts are seriously modified so as to constitute inflammation."

Under the head of the pneumogastrics we have a full description of the depressor nerve of the heart, recently discovered by Ludwig and Cyon. The origin and direction of this nerve are as follows: "In the rabbit is a nerve arising by two roots, one coming from the trunk of the pneumogastric and the other from its superior laryngeal branch, passing then toward the carotid artery and taking its course down the neck by the side of the sympathetic as far as the thorax. In the chest it joins the sympathetic filaments, to pass with them to the heart by little branches between the origin of the aorta and the pulmonary artery." Now, if this nerve be isolated in the neck from the sympathetic and pneumogastric, and then divided, galvanization of the central extremity has a twofold effect, viz., to diminish the arterial pressure (by a relaxation of the capillaries), and reduce the frequency of the pulse. We have not time to enter fully into this subject, but may be permitted to point out a seemingly important consequence. As the above effect does not take place when the peripheral extremity is stimulated, the action is a purely reflex one. Now, it is easy to see how, when the heart is overloaded from any cause with blood, the stimulation of this nerve would act beneficially in first diminishing the frequency of the heart's action, and secondly by diminishing arterial pressure; which latter effect would afford a free exit to the blood, and relieve the distention of the heart, until the proper equilibrium in the circulation should be reëstablished.

All the evidences regarding the structure of the spinal cord and encephalon, according to the most recent and trustworthy authorities, have been carefully studied by Dr. Flint, and a clear, if not altogether satisfactory, statement of our positive knowledge upon the subject given.

Dr. Flint takes issue with some late physiologists who have been inclined to favor the opinion that the spinal cord, when separated from the encephalon, is capable of perception and volition. The argument in favor of the older view is restated, and seems unanswerable.

Under the title "A Table of Weights of the Encephalon," etc., forty-six cases are reported of individuals in whom not only the weight of the encephalon is furnished, but in whom the degree of intelligence was more or less accurately known. We believe this to be the most complete list ever given, and furnishes a curious commentary upon the popular faith in "big brains." Of well-authenticated cases, following Cuvier and Abercrombie, who head the list, we find Ruloff and James Fisk, Jr. Among idiots and imbeciles at the extremity of the list we notice "a celebrated mineralogist," the weight of whose encephalon is given at 43.24 oz. A shock is given our childhood's faith by the statement that Daniel Webster's brain weighed 53.5 oz., when we had heard in our early years with patriotic pride that the old statesman's encephalon was only overmatched by that of Cuvier. We are still inclined to believe—for it is hard to give up one's early faith—that Webster's brain did weigh 63¾ oz. at the time he made his great speech against Robert Y. Hayne, though we are willing to admit that it must have sadly dwindled in the later days of the "compromise measures." In Dr. Flint's list we notice, conspicuous for its absence, the weight of the brain of Prof. Simpson, which was given, we believe, at the time of his death, at 53 oz.

Dr. Flint has discovered in his researches a curious case of aphasia, detailed in 1766 by Pourfour du Petit (p. 351). The description of the disease, at that time without a name, is complete, and in the *post-mortem* examination the location of the lesion in the left anterior cerebral lobe was fully recognized.

We think that few, at all conversant with the present state of the question, but will indorse the author's language upon the subject of locating the faculty of articulate speech. "Taking into consideration," he says, "all of the pathological facts bearing upon the subject, it seems certain that, in the great majority of persons, the organ or part presiding over articulate language is situated at or near the third frontal convolution and the island of Reil in the left anterior lobe of the cerebrum, and mainly in the parts nourished by the middle cerebral artery" (p. 353). The more the question is studied, the more overwhelming is the testimony in favor of this view.

One of the most interesting things in this volume is the author's analysis of "Andral's ninety-three cases of Disease of the Cerebellum" (p. 373). Dr. Flint regards the cerebellum as the organ of muscular coördination, but Andral's cases have furnished the principal ammunition for those who seek to discredit the coördination theory. The analysis of these cases shows that only three out of the whole number have any bearing upon the question, and that they in reality confirm the cerebellar doctrine of coördination.

Much has been said of late upon trophic centres and nerves. The facts are these: In progressive muscular atrophy, we have a gradual wasting of the muscles, uncomplicated with paralysis. The lesion in this disease consists in the destruction of certain cells in the antero-lateral portions of the cord, with more or less atrophy of the corresponding anterior roots. Now, the theory is, that these nerve-cells and the nerve-fibres emanating from them exercise a special influence upon nutrition, and hence are termed trophic cells and nerves in contradistinction to those which have motor or sensory functions. To the cutting off of the influence of these cell-centres is attributed the wasting of the muscles in progressive muscular atrophy. But Dr. Flint argues, and it seems to us with perfect justice, that it is entirely unnecessary to invent a new order of cells to account for the phenomenon. "It is a fact," he says, "that, when a muscle or a part of a muscle is deprived of motor stimulus, its fibres atrophy, become altered in structure, and lose their contractility." The gradual and progressive degeneration of a few of the motor cells would lead to the progressive and partial paralysis of the muscles to which their nerves are distributed. Now, if this would take place from the degeneration of motor cells, which are known to exist, it surely is a work of supererogation to go further, and to imagine the existence of cells which are in their nature purely hypothetical, and beyond the range of exact demonstration.

The work concludes with a chapter on sleep, the theory of which need not detain us.

To those who love careful, honest, and conscientious work as contrasted with the ordinary indecorous book-making, which serves no earthly purpose except to contribute to the vain-glory of the author, we cordially commend this volume.

ART. II.—*Injuries of Nerves and their Consequences.* By S. WEIR MITCHELL, M. D. Philadelphia: J. B. Lippincott & Co., 1872, pp. viii.–377.

BY direction of Surgeon-General Hammond, to whom this book is appropriately dedicated, the United States Army Hospital for Nervous Diseases and Injuries was established in May, 1863, and Drs. Mitchell, Morehouse, and Keen, were appointed as its staff. Following this precedent, and with a wise desire to promote the interests of scientific medicine, Dr. Hammond established numerous hospitals or wards for the study of various special diseases. Nothing that he did has reflected more credit upon him in its widely-useful results. The book before us, which may be regarded as a second, much enlarged, and improved edition of an earlier work ("Gunshot Wounds and other Injuries of Nerves," Philadelphia, 1864), by Dr. Mitchell and his colleagues, is the evidence how thoroughly the unusual advantages offered by such a hospital and by subsequent private and hospital practice have been used. No other such treatise on the nerves exists in any language, and it is an honor to the profession in America, not only because it stands without a rival in time, but also because it is so thorough, exact, and exhaustive. It lacks but one element to make it complete—and this also is a credit to its author—

the confirmation of many of its views by a *post-mortem* examination. But one case died in the seventy related in full (that one involving the brain), though there are some pathological results derived from operations.

Dr. Mitchell opens with two chapters on the anatomy and physiology of nerves. Nowhere is his clear descriptive power better exhibited, and we do not know any more concise yet exact summary of our knowledge of the nerves than is to be found here. The experiments of Tillaux and Dr. Mitchell on the stretching of nerve-trunks, amounting in some cases to one-fifth of their entire length before their functions suffered annihilation, are very noticeable, as also those on the chiasms of the cutaneous nerves at the median line, suggested by the author's discovery of the chiasm of the superior laryngeal nerves in the chelonia.

The much-vexed question of the existence of independent trophic nerves, and also of nerves of temperature, pain, etc., is discussed at length, and in each case their existence is denied. That nutrition and sensation, whether common, thermal, or of pain, are dependent on the integrity of the nerves is evident, but, until we are able to obtain clearer views of the nature and action of the nervous force itself, and establish either the identity or want of identity between the motor and sensory nerve-fibres, it is hardly to be expected that we can add to their varieties with confidence. It would seem more probable that the various specific differences of impressions conveyed by the nerves are rather correlated varieties of one force than various forces acting through as many different kinds of nerves.

In the chapter on the pathology of nerve-lesions, the "congestion of nerves," a subject hitherto much neglected, indeed by most practitioners and writers entirely ignored, has a deal of light thrown upon it. Following up his idea of investigating the nervous system by cold, which he has formerly used with such advantage, he has established the symptoms of this condition most clearly. His experiments on this subject, and also on the effects of contusion, show how earnestly he has endeavored, by an appeal to the lower animals (in spite of the S. P. C. A.), to overcome the want of pathological research in his more fortunate patients.

Following the "Mechanical Injuries of Nerves" we have three chapters on "Symptomatology." Prominent among them is that of "Shock," the theory of which is discussed at some length. The view the author first proposed, of exhaustion of ganglionic centres, is again upheld in opposition to that of vaso-motor spasm, as proposed by Brown-Séquard. How wide his opportunities for enlarging both his practical and his theoretical knowledge have been, may be seen from the fact that under the head of wounds of nerves alone he has added forty-eight cases to the forty-three which were analyzed in his former work with his colleagues.

The peculiar "burning pain," and the trophic changes in the skin and its appendages, are described with great exactness, though but little has been added to his former admirable descriptions, except the remarks on growth of the nails as a means both of diagnosis and of prognosis. In a later communication (*Philadelphia Medical Times*, June 1, 1872), he has called attention still more prominently to the value of this means of diagnosis between cerebral and hysterical paralysis. The view which his distinguished father proposed in 1831, that arthritic rheumatism was connected with affections of the spinal centres, and which, in 1864, he recalled to the attention of the profession, is again shown to have additional evidence in its favor.

The chapters on treatment are full, concise, and practical. Galvanism naturally and properly holds a high place in his esteem. We notice that he advocates the use of sutures in nerve-wounds, but suggests that the sutures be carried through the sheath of the nerve rather than its body. The question of excision is considered fully, and a complete table of all the cases thus far recorded (twenty-four in number) is given.

He concludes with a valuable chapter on the neural maladies of stumps, a subject which, strangely enough, was totally neglected during the late war, though the opportunities for careful observation were so numerous. The well-known phantom-limbs of those who have suffered amputation, and many other peculiarities of stumps hitherto overlooked, are fully described. Unfortunately, he has omitted to call attention to the researches of Dickinson and Vulpian on the degeneration of the nerves and the spinal cord which follows amputation.

Dr. Mitchell's experience as a writer, both in medicine and in general literature, is quickly seen in its influence on his style. Clear, terse, vigorous, and straightforward, he describes what he has seen with unusual precision. His arguments are never prolix, but are always well put, even if not convincing. By long practice as an experimental physiologist, his inventive genius has been unusually developed; and no one can read the book without admiring not only his industry, but also the fertility of his resources.

The book is singularly free from misprints, and those very slight. On page 88 we observe the only serious one of "labial" for "radial." We regret also that Dr. Mitchell has not added the bibliography of the subject. It is all that is needed to make his book a complete monograph.

ART. III.—*A Treatise on Diseases of the Bones.* By THOMAS M. MARKOE, M. D., Professor of Surgery in the College of Physicians and Surgeons, and Surgeon in the New York Hospital, Bellevue Hospital, Roosevelt Hospital, etc. New York: D. Appleton & Co., 1872.

THIS book is a handsome octavo of four hundred and sixteen pages, embellished with one hundred and ninety-two engravings, thirty of which are original, the volume throughout being in the best style of the Appletons.

The well-known character of Dr. Markoe would lead one to anticipate a good book, and in looking over these pages that expectation is not disappointed—provided the modest preface has been read at the outset.

Proceeding by a jump from the title to the contents, one might be surprised at the absence of much required to make this book the successor of Stanley's, written now more than twenty years ago. The author explicitly disclaims in the preface any attempt to make his book a complete compendium upon the subject, and states that, in the arrangement of the work, he has "followed rather the leadings of his own studies and observations."

Judging the book by its original intent, it is a decided success; its style is marked by the terseness and perspicuity natural to the author, and to those who, from "the leadings of their own studies and observations," desire a good book upon this subject, we most heartily commend it.

The order of the book is somewhat similar to Mr. Stanley's; the first chapter is devoted to diseases of bone, the second to tumors of bone, the third to malignant diseases of bone.

The chapter on the Malignant Diseases of Bone, both for its scope and thoroughness, is exceedingly good. The various methods of treatment are freely discussed, while the opinions of the author are unreserved and definite. A wholesome conservatism is manifest in the conclusions of the author, the sum of which concerning ablation are—

1. That operation does not cure, but merely palliates.
2. That incomplete operation hastens the fatal termination.
3. That the operation itself adds largely to the dangers of the patient's condition.

The chapter on Tumors is very thorough, learned, and complete. It presents an excellent *résumé* of the literature of the subject, and is freely supplied with familiar illustrations from Paget, Billroth, and Heath, with others by the author. Tumors of the jaws are treated with a completeness not surpassed by any book in the English language. The chapter on "Diseases of Bone," which is first in order of arrangement, and which is preëminently the chapter which the practitioner would most frequently require for reference, seems to lack certain details of treatment which, if given, would greatly enhance its value. "Chrônic Sinous Abscess of Bone" is the title of a section to which are devoted thirteen pages, the chief purport of this section being to establish a distinction between the ordinary limited phlegmonous abscess and that which, for want of drainage, burrows, reproducing itself successively as the inflammation continues. Whether this distinction is great enough, or constant enough, or important enough, to justify the title the author has given it, will be a matter for the future experience of others, conjointly with his own, to determine. While thirteen pages have been devoted to this point, it seems strange that the whole subject of tubercular disease of the bone is disposed of in nine pages. In view of the fact that the various diseases coming under this head are not only so commonly presented for treatment, but are also more amenable to it than others to which so much space has been devoted—in view of the further fact that the mechanical appliances for the treatment of some of the cases of this class of diseases have been so ingenious, so numerous, and so successful, as to mark a new era in this department of surgery within but a few years past, one cannot but deplore that the author chose so to limit the scope of this book as to pass these methods of treatment without a mention. For the treatment of this class of diseases the reader is simply referred to the chapter on Caries, where we find, as the sum of the means for securing local rest, that old and fatal rule of by-gone days epitomized thus: "The bed is the only security against injurious and painful motion." The all-important and cardinal principle of local rest, with unlimited means of constitutional invigoration, is left unmentioned; but, as before said, that would have evidently overstepped the limits the author prescribed for himself. While measured by his purpose, the author has accomplished a handsome success; had the plan of the book been based less upon the inclinations of the author, and more upon the wants of the profession, the author would have lost nothing, while the profession would have gained more.

ART. IV.—*Experiments with Reference to ascertaining the Influence of Morphine, Quinine, and Arsenic, upon the Decomposition of Albumen in the Animal Economy.* By Dr. HERMANN VON BOECK, München, 1871.

THE writer, in a pamphlet of fifty-two pages, gives a detailed account of a single series of experiments upon the same dog with each of the above-mentioned substances, including a carefully-prepared table of the chemical analysis of the urine and fæces for several days previous and subsequent to, as well as during, the experiments. He concludes, from his first experiment, that "the decomposition of the nitrogenized substances in the animal organism is only to a very inconsiderable extent diminished by the action of morphine."

The experiment with quinine extended over a period of eleven days, during which time (as had been done in the case of the morphine) the urine and fæces were carefully analyzed. The result of his experiment led Von Boeck to decide that "quinine is able to diminish, to a greater degree than morphine, the quantity of nitrogen given off by, and the decomposition of the albuminoid substances in, the organism; but still to a very limited extent only."

In the experiment with arsenic, Von Boeck observed the excretions during a period of sixteen days, and asserts that "the employment of arsenic exerts no essential influence upon the amount of nitrogen given off, nor upon the decomposition of the albuminoid substances."

Books and Pamphlets Received.—Sur un nouveau Dissolvant de l'Iodure plombique et de son application à la Pharmacie. Par Donato Tommasi, docteur ès Sciences. Mémoire présentée à la Société de Pharmacie de Bruxelles. Paris, 1872, pp. 8.

Diseases of the Throat: A Guide to the Diagnosis and Treatment of Affections of the Pharynx, Œsophagus, Trachea, Larynx, and Nares. By J. Solis Cohen, M. D., Lecturer on Laryngoscopy, and Diseases of the Throat and Chest, in the Jefferson Medical College, Philadelphia, etc. With One Hundred and Thirty-three Illustrations on Wood. New York: William Wood & Co., 1872.

Hysterology: A Treatise, Descriptive and Clinical, on the Diseases and Displacements of the Uterus. By Edwin Nesbit Chapman, M. A., M. D., late Professor of Obstetrics, Diseases of Women and Children, and Clinical Midwifery, in the Long Island College Hospital. New York: William Wood & Co., 1872.

On Winter Cough, Catarrh, Bronchitis, Emphysema, and Asthma: A Course of Lectures delivered at the Royal Hospital for Diseases of the Chest. By Horace Dobell, M. D., Senior Physician to the Hospital. New and enlarged edition, with colored Plates. Philadelphia: Lindsay & Blakiston, 1872.

A Manual of Qualitative Analysis. By Robert Galloway, F. C. S., Professor of Applied Chemistry in the Royal College of Science for Ireland, etc. From the fifth rewritten and enlarged London edition. With Illustrations. Philadelphia: Henry C. Lea, 1872.

Case of Excessive Hypodermic Use of Morphia. Three Hundred Needles removed from the Body of an Insane Woman. Reported by Judson B. Andrews, M. D. From the American Journal of Insanity for July, 1872.

A Nomenclature of Diseases, with the Reports of the Majority and Minority of the Committee thereon. Presented to the American Medical Association at the Meeting held in Philadelphia, May, 1872, pp. 94.

The Magnetic and Mineral Springs of Michigan, to which is prefixed an Essay on the Climate of Michigan, by Styles Kennedy, M. D. Wilmington, Delaware: James & Webb, 1872.

Catalogue of the Graduates and Officers of the Medical Department of the University of the City of New York. Published by the Alumni Association, July, 1872, pp. 113.

Syphilis: its Nature and Treatment. With a Chapter on Gonorrhœa. By Charles R. Doysdale, M. D., M. R. C. L., etc. London: Baillière, Tindall & Cox, 1872.

Modern Medicine: its Status in Modern Society. By Homer O. Hitchcock, A. M., M. D. The President's Address before the Michigan State Medical Society.

Ophthalmic Hospital Reports, and Journal of the Royal London Ophthalmic Hospital. Edited by J. F. Streatfeild. No. XIII. London: Wm. Churchill, 1872.

The Ten Laws of Health; or, How Disease is produced and can be prevented. By J. R. Black, M. D. Philadelphia: J. B. Lippincott & Co., 1872.

Transactions of the Twenty-first Anniversary Meeting of the Illinois State Medical Society, held at Peoria, May 16, 1871, Chicago, 1872, pp. 160.

Revue Photographique des Hôpitaux de Paris. Bulletin Médical publié par Bourneville et A. de Montmeja. June and July Nos., 1872.

The Origin, Progress, and Present Position of the New York Society for the Relief of the Ruptured and Crippled. New York, 1872.

Le Siége de Paris (1870–'71). Historie d'une Ambulance par le Dr. A. Bertheraud. Paris: J. B. Baillière et Fils, 1871, pp. 55.

Braithwaite's Retrospect of Practical Medicine and Surgery. Part LXV., July. New York: W. A. Townsend, 1872.

Half-Yearly Compendium of Medical Science. Part X., July, 1872. Philadelphia: S. W. Butler, M. D.

Reports on the Progress of Medicine.

SURGERY.

Clinical Lecture on Lithotomy. By Prof. HUMPHREY, F. R. S. [Lancet, June 1, 1872.]

THE great secret of success in operations, as in all other efforts in life, is a painstaking and careful method of procedure; and in no operation is this more true than in lithotomy. In it the recovery of the patient depends, perhaps more directly than in any other, on the manner in which it is performed. Accordingly, when, thirty years ago, having just emerged from the period of pupilage, I had the good fortune to be appointed surgeon to this hospital, where I knew I should often be called upon to perform lithotomy, I was much impressed with the responsibility of the task, and set about seriously to consider the mode in which it could best be done; and I laid down for myself a plan to which I have ever since closely adhered. A careful investigation of the subject and a consideration of the different modes of operation practised by the most eminent men convinced me that in this, as in most operations, the minimum of deep cutting was the safest course and gave the best results. Reading confirmed what observation had shown me, and what reason had suggested, that, not the brilliant, but the cautious operators, were the most successful. I came to the conclusion that in deeply-seated parts it is better to make nineteen or twenty cuts, amounting in the aggregate to nine-tenths of an inch, provided that is sufficient, than one cut amounting to an inch. The extra tenth will, now and then, turn the scale against the patient, though the quicker proceeding may seem to tell in favor of the operator.

You saw, on Friday last, the plan I am in the habit of following. The patient, a middle-aged, healthy man, was admitted on the previous Wednesday, and had not therefore undergone any preparatory treatment, which I think as often does harm as good by depressing the spirits and lowering the bodily tone. He was not very clear as to the duration of his symptoms, but it appeared that they were not of long standing, and the sounding indicated the stone to be small. Why, then, did I not perform lithotrity? I believe it would have been a good case for it, and I was half disposed to do it. But a good case for lithotrity is one in which lithotomy, carefully performed, is attended with very little risk, with quite as little, I believe, as lithotrity. It is less painful, quicker, and more certain; and the greater number of our patients prefer this more expeditious process. I use the common staff with the groove on the convex side, in preference to one in which the groove is placed laterally, as recommended by Mr. Liston, because the groove, when on the convex side, is more easily felt in the perinæum, and the knife is more easily pressed into it and is less likely to escape from it. A common scalpel is quite as good as any of the variously formed knives which have been employed in this operation; and it is advisable to adhere to a few instruments, just as it is advisable to adhere to a few remedies. By using many of either we merely dilute and waste our experience. The man who uses a few weapons is more likely to wield them skilfully than he who tries, and so perplexes himself with, many. Having made a tolerably free incision through the skin and superficial fascia, I pushed my left forefinger through the tissue of the perinæum, down upon the staff a little behind the bulb, so that I could feel the groove of the staff sufficiently to guide the point of the knife upon my finger-nail into the groove. I then cut a little way along the staff, withdrew the knife, examined with my finger, and, again introducing the knife as before, cut a little more, repeating the process till the part of the prostate nearest to the surface was divided, and till I could insert my finger, alongside the staff, through the prostate, into the bladder. Thus the opening was dilated. It was further dilated by my passing the forceps, beside the finger, into the bladder, which I did after having explored the bladder and felt and ascertained the size of the stone. I make a point of doing this, as it greatly facilitates and gives safety to the further steps of the operation, and, having the advantage of a long finger, I am almost always able to examine the size and position of the stone, and select the forceps accordingly. These should be so made as to hold the stone securely and not let it slip from between the blades; and the blades should be broad, so as to cover the stone and prevent its sides catching and rubbing roughly against the tissues as it is drawn through the wound. In the present instance the forceps did not open easily, in consequence of the hinge not being quite free. To have used force in opening these might have endangered the wall of the bladder; so I at once withdrew them and resorted to the scoop, which I guided upon the finger again introduced, and with which the stone was quickly removed. An elastic tube, guided upon the finger into the bladder, was secured there, and removed on the third day. The man has not had the slightest pain or discomfort since the operation, and is doing well.

The points on which I lay most stress are, the pressing the finger down upon the staff in the membranous part of the urethra, and guiding the scalpel by it into the groove, the careful cutting into the prostatic part of the urethra, and the completing the operation with the finger and the forceps or the scoop. I rarely do more cutting than this. If it is a large stone, I divide the prostate a little more freely with the knife, and then allow the wound to be dilated by stretching or tearing of the tissues as the stone is drawn through it, which I do slowly and steadily, though I have sometimes to use considerable force. By this plan there is no greater division of the tissues than is absolutely necessary, and there is less danger of ill consequences than if the knife is resorted to. I ought to add that it is necessary to be very careful not to allow the finger, still less the knife, to stray into the loose tissue on the exterior of the prostate. Particularly is this care needed in operating upon children. I am sure that many of the disasters which have occurred in the operations upon young subjects have been caused by the finger pushing its way on the exterior of the prostate and bladder, where the tissue easily gives way before it, and where a cavity is easily made by the finger, which the operator mistakes for the bladder. Into this he plunges the forceps, perhaps seizes the bladder or the rectum, or does some fatal mischief. In a little child it is sometimes difficult to enter the bladder. The tissues are soft, and readily give way before the finger; the bladder seems to be at an unexpected distance, and the operator is unnerved by the difficulty. I can give no more valuable hint than that of preparing a young operator for this difficulty, and of warning him to be content to go slowly on, not to be seduced into quitting the guidance of the staff, not on any account to withdraw the staff till he is certain that his finger is in the bladder, and till he feels the stone with it, but to work quietly and gradually on with finger and knife till he has brought the finger into contact with the stone. Indeed, it is a good rule, from which I hardly ever depart, not to allow the staff to be withdrawn from the bladder till the finger is upon the stone, and not to withdraw the finger till the forceps or scoop is in the bladder. I do not use a very large staff, because it increases the difficulty of passing the finger along it through the prostate. I introduce it with a slight wriggling, rapid, but gentle movement. I am not satisfied unless I hear it strike the stone as well as feel its contact with it. The ear is in this case a better guide than the finger; and, if this were more attended to, the cases in which a stone is not found would be fewer. The touch is often deceptive, especially in children, in whom the projecting and slightly-roughened rugæ of the bladder frequently communi cate, when the sound is passed quickly over them, a sensation to the finger like that given by a stone. I was present not very long ago when an operator opened the bladder and could not find a stone. To my inquiry, two or three times repeated, "Did you hear the staff strike upon the stone before commencing the operation?" the reply was, "Oh, we felt the stone quite distinctly; there was no doubt of that."

Rules in surgery, however, must sometimes be departed from, and judgment and skill may be more shown in the breaking than the observing them. A lad was in the hospital two years ago with severe symptoms of stone, the urine constantly passing. The sound appeared to grate upon something, but we could not hear it strike a foreign body. Several trials gave the same result. The symptoms were so bad that it seemed best, at any rate, to open the bladder and search for the cause. Accordingly, having introduced the staff as usual, and perceived the grating, I cut carefully upon it and along it quite to its end. I could not discover a stone, and I soon became certain that neither my finger nor the staff was in the bladder. The staff, being no longer of any use, was withdrawn. Then my finger touched the end of a rough stone. Supposing that this had escaped from the bladder into the surrounding tissue, I was endeavoring carefully to extract it when it disappeared; and my finger, following it, passed through what was evidently the prostatic part of the urethra into the bladder, and felt the stone there. It was removed without difficulty. It is, as you see, composed of two oval parts, united at an angle of about 100 degrees. Of these one has a nucleus of lithic acid, and lay in the bladder; the other consists of phosphate of lime, and lay in a cavity or urethral pouch outside the bladder; and the bent neck joining the two occupied the prostatic part of the urethra. The staff, no doubt, had passed into the urethral pouch, beneath the calculus, had probably penetrated the delicate wall of the pouch, and found its way into the cellular tissue behind the bladder, into which I travelled along its groove with scalpel and finger. The stone lying in its concavity was not discovered by my finger till the staff was withdrawn. The boy quickly recovered.

You observed, last Friday, that I quickly relinquished the forceps, and resorted to the scoop. This is an excellent instrument and very safe, because it does not go much beyond the range of the finger. It is not trusted in the bladder without the finger, and is not opened and shut and moved about in the bladder so freely as the forceps are; and, with a little practice, the stone is very easily caught between it and the finger, and drawn through the wound. Even large stones may be thus removed. This stone weighed eight ounces. It is the largest I have ever seen extracted by lithotomy. It is nearly spherical, is rough on the exterior, and was grasped by the bladder so that I could not lay hold of it with the forceps, and should probably have failed to complete the operation had I not fortunately taken with me (it was several miles from Cambridge) this strong, well-bent scoop. I contrived to insert it between the bladder and the stone; and it held to the stone so well that with it and my finger I succeeded in gradually drawing the stone through the wound. The patient, a stout farmer, aged sixty-five, recovered without any unfavorable symptom. The handle of the scoop should be large and rough, and the stem round and strong. In some of these scoops I have had the bowl set sideways on the stem, which sometimes facilitates the catching of the stone between it and the finger.

With regard to hæmorrhage: I am in the habit of telling my pupils that hæmorrhage after an operation is almost always attributable to some fault on the part of the surgeon. It indicates that he has not been sufficiently careful in searching for the bleeding vessels, or that he has not properly secured them. This is, at any rate, a very safe doctrine to inculcate; and it applies to lithotomy as well as to other operations; for the vessels which bleed most briskly, and are most likely to give trouble after the patient is placed in bed, are commonly within reach, and may, with care, usually be found and tied. They are the artery of the bulb, or the transverse artery, or some branch divided near the pudic; any of which may be tied. After the removal of the stone I always attend to this point, and sponge out and explore the wound carefully, if there is any bleeding which is likely to give trouble. When the hæmorrhage is from a deeper source than I have indicated—that is, from the neighborhood of the prostate—it cannot always be arrested; but in such cases it usually ceases spontaneously after a short time, there not being commonly any arteries in this situation sufficiently large to keep up, or to cause a return of, the bleeding.

I leave an elastic tube, passed through the wound, in the bladder, in men, to avoid the difficulty which is sometimes experienced by the patient in voiding urine after the operation. I do this because I was, in several instances, summoned, after a few hours, in consequence of the patient being in severe pain and unable to pass urine; and I was obliged to introduce my finger into the bladder to afford relief. This trouble, which was perhaps a consequence of the small size of the incision I am in the habit of making into the bladder, is quite prevented by the tube. The tube may be removed in two or three days, or earlier if desirable, and is not again required.

In one instance—a gentleman, aged sixty, in whom the operation was unattended with any particular circumstances—there was rather severe inflammation of the bladder ten days after the operation, with discharge of tenacious and semi-purulent mucus. For several days I introduced, two or three times a day, an elastic catheter by the urethra into the bladder, and injected through it warm water, which, passing out at the wound, washed out the bladder. The symptoms subsided; and he was soon quite well, and has remained so. In another case, when the wound was healing, the patient, a farmer, aged sixty-four, had retention, and required the catheter to be introduced two or three times daily. After a time he quite regained the power. In a third patient, a middle-aged man, in the hospital, the symptoms of bladder irritation set in while the wound was healing, and increased in severity, so that I introduced a sound, and, discovering a calculus, I reopened the wound, partly with the knife directed by the staff, and partly with the finger, and removed a rough, phosphatic calculus, which I have no doubt had formed in the interval since the first operation, when the stone removed was lithic acid. I mention these cases because it is possible that the symptoms may have been dependent upon the incision in the prostate having been small, and the parts having, in consequence, sustained some injury in the extraction of the stone. I do not know that it was so; and, if it was, the occasional occurrence of such events would not induce me to deviate from the practice I am in the habit of pursuing.

With regard to the after-treatment, it is very seldom that any is required. The diet should be, not according to any rule, but regulated by the condition and the desires of the patient. He usually prefers to limit himself to tea, broth, etc., for a day or two; and this generally suits him best. Then appetite returns gradually, and may be yielded to. We do not often find that wine is necessary. The man now under treatment, as I just said, has had no pain or discomfort whatever. Generally some pain is experienced when the urine flows, or at other times; but whether there is much pain or little, whether the patient sleeps or does not sleep, I refrain from giving opium or any other sedative. Pain, even severe and long continued, though hard to bear, does not seem to do the body much harm. I have often been surprised how little wear and damage it does—not so much, I think, as the sedative which is given for its relief; and the exhaustion of a sleepless, restless night or period is usually followed by sleep, which is more likely to occur naturally at an early period if sedatives have not been used. Under ordinary circumstances a restless night is less damaging than a night of sleep induced by a sedative. The patient commonly wakes from the latter paler, weaker, lower, more enervated, and less able to bear pain. I have long had this conviction; indeed, throughout life I have acted upon it. It may be regarded as a prejudice; but I think the patients recover better and quicker, and are less liable to pyæmia, erysipelas, and other unfavorable sequelæ of operations, when thus treated. These, at any rate, we rarely see in Addenbrooke's Hospital; and an immunity from them is, I think, partly due to the practice of non-interference after operations, so generally followed in this hospital. You will understand that I make a distinction between the use of opium as a remedy for disease, and as a means of relieving pain after operations. In the former case it is a very valuable medicine under many circumstances, one of the most valuable we possess. But in the latter case there is no disease; the pain is merely a natural consequence of the physical injury which has been sustained, or of the local changes which are taking place as a consequence of the injury. The condition will generally subside after a short time; and it is much better to allow it to do so, unless there is some special reason for interference. It is well to suspend the scrotum, especially in elderly persons, so that it does not become moistened with the urine, to keep the parts as dry as possible, and to defend them by the occasional use of oil. I do not remember any instance in my practice in which the wound failed to heal soundly within a few weeks, or in which any unpleasant symptom, except slight difficulty in holding the water, followed; or any case in which the rectum was wounded, or in which any troublesome local symptom remained.

Miscellaneous and Scientific Notes.

Secondary Hæmorrhage; its Causes and Prevention. (From Prof. Billroth's "Letters from the Army Hospitals," 1870. Translated by CHARLES E. HACKLEY, M. D. Continued from page 200.)—It is correctly urged that hæmorrhage is very rare when the ligature is applied to a divided artery after amputation or in a large wound. After sad experience with ligating in the continuity, it was thought it might be rendered more certain by applying two ligatures, and dividing the artery between them. In explanation of this difference it was urged that, in an artery ligated in the continuity, the thrombus formation was not so complete as in a divided artery which was retracted and had its intima more folded. A comparison of the different results, after ligating large arteries in amputation-wounds and in the continuity, induced the belief that in the latter case the central end of the artery is guarded by the stronger thrombus.]

But few cases in literature show undoubtedly whether the lower or upper end, or both, bled after separation of the ligature.

From my few observations I cannot accept the view that the peripheral end of arteries ligated in the continuity is more liable to secondary hæmorrhage, and think that the facts in regard to this question should be more definitely determined, especially as regards the ordinary points of ligation of different arteries.

I do not know whether there are any physiological grounds for the formation of thrombus being more difficult at the peripheral than at the central end.

But the permanent closure, the growing together of the ends of the artery after ligation in the continuity, must from some cause be more difficult than after ligating in an amputation, else so many secondary hæmorrhages would not occur in the former case. The cause for this has hitherto been sought only in faulty development of the thrombus. If we consider also the healing of openings in the arteries without thrombus, we may perhaps also find causes that act unfavorably in ligations in the continuity. This might depend somewhat on the mobility of tissue around the artery. The granulations of the surrounding tissue and the surface of the artery should grow over the ligated artery and contract before the ligature is fully detached. Hence, those points are particularly unfavorable to ligation, where the artery is surrounded by firm tissue which is but slightly movable or lies next to a bone. According to this, the subclavian, where it bends over the first rib, the femoral, just below Poupart's ligament, and in the tendinous canal of the adductor muscle, as well as the popliteal artery, should be peculiarly disposed to hæmorrhage after ligation, and experience would seem to confirm this view. We must also remember that certain movements especially disturb the healing of arteries: vigorous motions of the arms, deep inspirations and expirations, coughing, sneezing, etc., may rupture the recent cicatrices covering the carotid or subclavian after detachment of the ligature; lifting the pelvis, particularly for putting under the bed-pan, may start a hæmorrhage from the femoral ligated at Poupart's ligament, etc.

Another thing said to favor secondary hæmorrhage is pyæmic poisoning. This probably has no effect on the firmness of the arterial walls, nor would it interfere with the formation of thrombus; for those who refer the secondary hæmorrhage to pyæmia, usually regard extensive venous thromboses as due to the same cause (in most cases I should regard them as due to periphlebitis). But, with my views I may admit that pyæmia favors softening and destruction of the thrombus, for under its influence almost all recent pathological neoplasiæ assume the character of pus; moreover, the granulations, shrunken, relaxed by pyæmia, are ill suited for the permanent closure of the vessel, especially if there be an imperfect thrombus or none at all.

In regard to the condition of the artery after ligation, I think that in most cases a thrombus is formed, but that in length and firmness of attachment to the arterial walls this varies much more than was formerly supposed. If the ligature separate early, and a firm thrombus has formed, it is a very favorable circumstance; it may arrest the bleeding, not only temporarily, but even till the opening in the artery has been firmly closed by granulations and cicatrix.

Experience about the uncertainty of the development of thrombus renders it very desirable to find other methods for arresting hæmorrhage. This is such an important question that we may review the various means for avoiding the fatal hæmorrhage following separation of the ligature. Some of the means aim at aiding, as much as possible, the formation of thrombi when ligating in the continuity; thus, the artery is not ligated in the immediate vicinity of a large branch, or, if this be necessary, this branch is also ligated. It is also recommended always to apply two ligatures, and to divide the vessel between them. Both proceedings are advantageous, but are not certain preventives against hæmorrhage.

After occasional experience in extensively laying bare the carotid artery and jugular vein, and the axillary vein and artery, I should deny that this exposure would cause an inflammation of the coats of the vessels and thus induce more extensive thrombosis.

Kocher relates an experiment ("Archiv für klinische Chirurgie," Bd. xi., p. 678) showing that irritation of the walls of the vessel may induce extensive thrombosis in an artery; this was excited in the femoral artery of a rabbit, after a ligature soaked in croton-oil had been placed around the adjacent vein; such a ligature around the artery would probably have had even more effect. I would not recommend the remedy—it seems too active, and is dangerous for the adjacent vein, as appears from a work by Dr. Gersuny, on "Cauterization of Recent Wounds in which Veins lie exposed." The mechanical attempts to induce thrombus in arteries have either been insufficient or rather dangerous. By applying a ligature firmly several times around an artery and removing it again, we may crush the intima, just as we may by compressing the vessel strongly with forceps; if the intima be injured in this way, a large thrombus will only form in the continuity of the vessel in case the injury has been extensive; but we are not certain that the artery will not occasionally become gangrenous, or aneurism develop at these maltreated points. I should hardly try such an operation.

A thrombus might also be caused by ligating an artery in the continuity, say the femoral, in the middle of the thigh; then thrusting two acupressure-needles at four lines' distance through the artery above, and other two below the ligature; in the course of forty-eight hours a coherent thrombus would form around them. This operation would not be applicable to all localities, but I should fear no evil results from it, unless the thrombosis were so extensive as to cause gangrene of the limb. It would be difficult to foretell whether a thrombus thus induced would become organized or last only a short time.

I have made numerous observations as to the duration of thrombi; most of those following ligation disappear again as I have often asserted; some of those in cavernous tumors and veins persist in the form of vein-stones, or occasionally as chalky corks. Arterial thrombi in arteritis with gangrene usually continue; I have sections of a thrombosed posterior tibial artery, in which are clearly deposited chalk and quantities of pigment. Probably the thrombus disappears rapidly when it becomes vascular early, otherwise it persists and petrifies.

There is another method of inducing thrombus, as we do by crushing the artery, without the danger of dividing it in the continuity: this is to expose the artery, rotate it on its axis, and fasten it in its new position without dividing it. For instance, the exposed femoral artery may be seized on one side, and rotated half, or, by a second forceps, entirely on its axis; this will arrest the current of blood; perhaps the artery might be fixed in this position by an acupressure-needle without its coats being cut through; after forty-eight hours the needle might be withdrawn, the artery being retained in position by the fibrine in the wound; then, whether a thrombus forms above and below or not, the vessel would still be occluded. Dr. Gersuny is now making trials of this method, which I think has already been proposed or even carried out by some one else.

Other operative procedures for ligating in the continuity do not aim at causing a thrombus, but try to effect a speedy closure of the wound, and with it of the artery. For this purpose it is advised to make the wound as small and smooth as possible, to avoid all stretching or bruising of the wounded surfaces, and to make the opening in the sheath of the vessel as small as possible, to have the ligature strong enough but not too thick, and to draw it moderately tight without rupturing the intima completely. These rules are well worth following; every thing should aim at healing the wound as quickly as possible.

To attain union by first intention, if possible, Hennen recommends cutting the ligature short and letting it heal in; it is difficult to say whether in such cases the ligature will always remain in the wound, or will subsequently suppurate out, for the silk now used is much more suited to that end than what was formerly employed. In one of my successful ovariotomies, fourteen silk ligatures were cut short and left in the abdomen, and have remained there for years. Healing of the wound by first intention is certainly one of the most important protectives against secondary hæmorrhage. Fine metallic wires might also be used as ligatures.

We may here also mention the use of substances that will subsequently be absorbed, such as cat-gut, etc., but thus far they have not come into general use, probably because they were found not to answer the purpose.

Two ligatures might be applied, the artery divided between them, then each end twisted several times, and kept in any desired position by acupressure-needles. We should thus have the advantages of the ligature (absolute safety, even if the patient were moved soon after the operation) combined with those of torsion. The needles might be removed in forty-eight hours, the ends of the artery would be retained by the fibrine, and subsequently by granulations and the cicatrix; should the ligature separate early, and the artery gape open, the torsion would still prevent hæmorrhage.

We come, lastly, to the proceedings intended *to close temporarily the arterial trunk supplying the bleeding part*, until a new thrombus has formed at the latter point, which shall be strong enough to close the opening in the artery till it has grown together. It is not intended to induce a thrombus in the main artery; hence it must be injured as little as possible. Continued digital compression is the best way, but the difficulties of this have been already shown. If the plan recently recommended by Adelmann, of compressing the artery by forced flexion of the extremities, were always as efficient as it has proved in some cases, it would be an excellent aid, but it can hardly be employed in broken limbs, and it is not always efficacious. In one case of popliteal aneurism, and in another of femoral aneurism, I tried forced flexion at the hip-joint, without arresting the pulsation. In a certain sense, torsion of the artery in the continuity would come under this head if the torsion were only made so far that the artery could go back again; the effect would be rather doubtful.

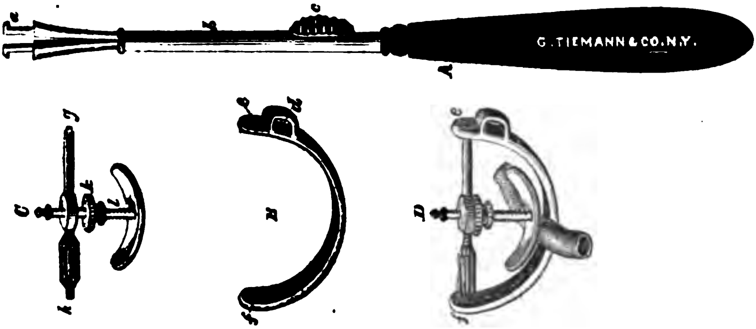
Efforts have long been made to effect a *temporary closure* of arteries by ligatures and instruments. For this purpose a ligature may be applied to the exposed artery, as it is in Ricord's subcutaneous ligation of the spermatic veins; or an acupressure-needle is passed under the artery and loop of suture thrown around; of course the latter falls off when the needle is withdrawn.

All of these ingenious methods have the common objection that by them the artery is more or less compressed by a needle or ligature, and the same dangers are induced as in ordinary ligature; this seems generally to be feared, else some one of the methods would have become more popular. To avoid making the pressure come directly on the vessel, percutaneous ligation and acupressure have been recommended: their use in small superficial arteries, such as the radial, cannot be denied; but a general resort to them in case of the carotid, subclavian, or iliac, is not imaginable, and it is about the large arteries that we are in dread.

Simon strongly urged a method which he called "immediate compression," on the principle of Theden's bandage, that is, making graduated compression over the exposed artery in a large part of its course. Simon relates a case where bleeding from the foot was thus arrested; but I cannot imagine its being of much use in gunshot-wounds of the thigh.

To cause temporary closure of an artery, we must find something that will best replace digital compression. The compressors used for aneurisms in the thigh or popliteal space would do good service in the army; attempts have been made to attain the same object by making an opening in a plaster-bandage where the artery may be compressed against the bone; in this opening a pad is placed, it is kept in place by the plaster-dressing, and is bound down by bandages running over the plaster, which prevents circular compression.

I now very timidly present an instrument[1] for the compres-



The handle A is attached to the semicircle B, thus: with the thumb resting on the projection *c*, the tube *b* is shoved forward, closing the elastic fork *a*, which is then passed through *d*, a hole in B, and the tube is again retracted, leaving A and B firmly united, and forming a *Deschamp's* ligature-needle. This is passed under the exposed artery, then the portion C is inserted into B, so that *g* fits in *e* and *h* in *f*; for this purpose *h g* is made to elongate by means of a screw, which fastens it firmly in B. By rotating the screw *k* on the rod *l*. this rod, with the semicircular portion at its end parallel to B, is brought toward B, and compresses the artery, as shown in D.

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[1] The accompanying cut is from an instrument made by Messrs. Tiemann & Co., 67 Chatham Street, and is half the natural size.

sion of the exposed artery; it may be applied from one day to three days, or even longer, without greatly injuring the coats of the vessel; it is intended to compress the exposed artery as we should do with two fingers. (So far as I know, this instrument is new.) The instrument is essentially a Deschamp's ligature-needle, which is inserted under the exposed artery; a compressor is placed in its concave side, fastened there, and the handle then removed.

I proposed that, instead of ligating the artery, this instrument should be applied and left on for a day or two (removing the handle, of course), and the screw then be relaxed; if hæmorrhage does not occur, the portion C and then B may be removed.

The instrument should be made very smooth and without angles, so as not to cause ulceration.

How long it may be left, without danger of gangrene of the compressed artery, must be determined by experience, also whether a thrombus will form above and below the instrument, whether this thrombus will be permanent, etc.

Pseudo-hypertrophic Paralysis. By Dr. PHILLIPP KNOLL. (*Med. Jahrbücher der k. k. Gesellschaft d. Aerzte*, 1872, 1.) Translated by S. G. WEBBER, M. D.—The author refers to the literature of this disease, and then relates a case. A boy, thirteen years old, whose parents were healthy and whose ancestors had no similar disease, first began to walk in his fourth year, and even then walked only with difficulty, yet he seemed well nourished and large in his legs. In his seventh year it was noticed that his calves were unusually large. From his seventh to his twelfth year he went to school, and made good progress in his studies. He then became unable to walk, or even to stand, without support. On May 30, 1867, he was sent to the hospital. The skin was of normal color; while he lay down, pupils reacted well and were of normal color. The upper extremities were of natural size, except the outer head of the triceps in both arms, which was enlarged, also the mass of muscles between the last ribs and the crest of the ilium was unusually prominent, as were also most of the muscles of the lower extremities. There was traction of the feet in extension and abduction. On standing, the skin of the lower extremities became marbled purplish. Sensibility to temperature, pressure, and touch, was normal. Testing the muscular reaction with the induced current, using a strength which called forth clearly-marked contraction in two healthy boys, caused also contraction in the hypertrophied muscles. Strong currents, which in the healthy boys, and in the muscles of the patient which were not hypertrophied, caused a quick and extensive motion, in the hypertrophied muscles produced only a slow and slight movement. Testing with the galvanic or battery current, it was found that eight-tenths more cells applied to the muscles, but six-eighths more applied to the nerves, were necessary to excite clearly-marked contraction, than on the corresponding muscles in the two healthy boys.

The average amount of urine in twenty-four hours was 1,676 cc. of 1.014 sp. gr. and containing 33.56 grammes urea. During another period of observation the amount was 1,739 cc. of 1.012 sp. gr., and containing 26.32 grammes urea.

He improved slowly without any special treatment. A portion of the left gastrocnemius was cut out; the wound healed very slowly. Examining the fresh muscle, by tearing its fibres apart, the fibres were seen to be of varying size; adhering to them, firm connective tissue; no fat either within or without the muscular fibres; the transverse striæ present, the distance between the striæ very small. The sarcolemma showed plainly by the action of acetic acid; no increase or division of the nuclei. Sections of portions, hardened in alcohol, showed the muscular fibres separated by wide tracts of firm connective tissue with few cells, but many very fine granules. In places this connective tissue seemed to have developed so as to cause a disappearance of the muscular tissue. In such places there was increase of small cell-elements. Nowhere was fat formed in this interstitial tissue.

According to their dimension the muscular fibres were divided into three groups. In one, the most numerous, were included those whose diameter was 48–66 μ; in the second, scarcely less numerous, 9–30 μ; in the third and least numerous group were included those measuring 75–120 μ.

He explains the firmness of the muscles by the absence of fat between their fibres, and the remarkable increase of consistency by the pressure of the fibres on the interstitial tissue during contraction, and the congestion of the cutaneous vessels by the same pressure. The cause of the contraction of certain muscles, he thinks, is to be found in the anatomical changes, the interposition of so much connective tissue between the fibres, and the bundles of fibres causing attraction upon the bones, and so the contraction; or, more probably, the connective tissue contracts like cicatricial tissue and so causes the abnormal position. Reference is made to the absence of fat between the muscular fibres. He believes the fat shows a later stage of the disease.

The author believes the changes in the muscular system to be dependent upon disease of the central nervous system, referring to the case described by Barth. The disease is hereditary, or may arise without such influence; the paralysis precedes the change in volume, and the two are not proportional.

The Nature of Putrid Infection.—Prof. I. Ravitsch, of the Medico-Chirurgical Academy in St. Petersburg, published in the year 1870 a monograph upon the "Nature of Putrid Infection," which, during the present year, has appeared in the German language. He has made a long and laborious and most intelligent series of experiments upon rabbits, sheep, and dogs, injecting into the veins hypodermically various kinds of decomposing animal matter, and has carefully watched and recorded the symptoms resulting therefrom. He has satisfied himself that, in the vast majority of cases, putrid blood is the cause of putrid infection. He has, therefore, used for his injections putrid fluid blood, putrid blood dried and dissolved in water, and putrid blood evaporated to dryness by boiling, and then dissolved in water.

From his experiments he believes himself justified in deducing the following conclusions:

1. Putrid infection is not wholly occasioned by *living* microscopic organisms, existing in the putrid substance; although we have no right to reject, as entirely without influence, a possible indirect effect of these organisms in the production of the poison.

2. Under ordinary conditions these microscopic organisms cannot survive in the blood.

3. The effect produced by the putrid substance is in proportion to the quantity injected.

4. There is no period of incubation in the action of this substance agreeing in this particular with the action of other poisons.

5. The sole pathognomonic symptom of putrid infection is the rapid molecular disintegration of the tissues with which the poison comes in contact, as also of the solid constituents of the blood, of the muscular fibres of the heart, and of the cells of the liver, spleen, and kidneys.

6. Especially, where such molecular disintegration occurs, do the microscopic organisms appear. In these cases it may be asserted with certainty that "decomposition begets decomposition."

7. Elevation of temperature, nausea, diarrhœa, convulsions, ecchymotic spots upon the serous membranes, considerable hyperæmia of the intestinal mucous membrane, with hæmorrhage into the intestinal tubes, cannot be regarded as characteristic indications of the presence of putrid infection.

8. A constant (though not characteristic) appearance in putrid infection is, stasis of the entire venous circulation, especially observable in the abdominal veins. Whether this has its origin in a paralysis of the vaso-motor nerves, or in insufficient force of the heart's action, resulting from the disintegration of its muscles, further experiments must decide.

9. Putrid infection cannot, like other infecting diseases, be produced by inoculation.—Henry D. Nicholl.

Appointments, Honors, etc.—A chair of Physiology has been established in the Maine Medical School, and Dr. Robert Amory, of Brookline, has been appointed lecturer. At the annual commencement of the University of the City of New York, held June 20, 1872, the following degrees were conferred on New York medical men: LL. D., upon Prof. Alfred C. Post, M. D.; Doctor of Philosophy, upon F. Leroy Satterlee, M. D. The trustees of the Massachusetts State Lunatic Hospital have appointed Dr. Bernard D. Eastman, of Washington, D. C., as

Superintendent, in place of Dr. Bemis, resigned. In the Académie de Médecine, of France, M. Tarnier has just been elected a member in the vacancy of the Section of Accouchements, caused by the death of M. Danyau. The chair of Operative Medicine, in the Paris Faculty of Medicine, rendered vacant by the death of M. Dénonvilliers, is not the only one that will have to be competed for. There is also that of Comparative Medicine, which has never been filled since the death of Rayer, but which M. Brown-Séquard has occupied temporarily *chargé de cours.* He has now resigned, and M. Vulpian, the present Professor of Pathological Anatomy, demands permission to change his chair for the one of Comparative Medicine. Dr. Lockhart Clarke has been invited to accept the post of Physician to the London Infirmary for Epilepsy and Paralysis, and has consented to do so. Mrs. Louisa Atkins, M. D., of Zurich, has been appointed Resident Medical Officer and Secretary to the Birmingham and Midland Hospital for Women.

Spasm of the Glottis. A. HAUNER. (*Journal für Kinderkrankheiten*, 1872, 1 and 2.)—The author compares the suddenness of the attack, and the instant succession of death, and the absence of congestion, either in brain, lungs, or heart, with the effects of the division of the medulla oblongata in dogs. Such results do not follow local hinderances to respiration, as œdema of the glottis, or strangling; nor are they found even when both recurrent nerves are divided. Various circumstances, which injure the quality of the blood and the nutrition of the nerve-centres, are predisposing causes; also as causes may be included unfavorable influences acting on the vagus and sympathetic, as over-distention of the stomach, or continuous errors in diet. In these cases, the attacks are reflex in their character. To these are added, by English physicians, the period of teething, and by Friedlieben, swelling of the thyroid gland; also enlargement of the bronchial glands pressing on the recurrent nerve, atelectasis of the lungs, and bronchitis, may bring on the spasm. He recommends tincture of musk with amber—two to five drops, twice a day, in sugar-and-water—with proper diet and regimen.

The Virtues of Eucalyptus.—At a recent meeting of the San Francisco Medical Society, the proceedings of which are reported in the *Pacific Medical and Surgical Journal*, Dr. Pigné-Dupuytren testified to the virtues of eucalyptus, which had been fully tested by himself and Dr. D'Oliveira, in the French Hospital. It had been found highly serviceable in affections of the larynx and of mucous membrane in general; also as a tonic. In France it is much used as a febrifuge, and by some considered a substitute for cinchona. The leaves growing near the trunk of the tree were the most aromatic. A large number of the trees had been planted around the French Hospital for sanitary and medicinal purposes. The leaves, in drying, emitted a large amount of balsamic exhalations, capable of causing headache in persons much exposed to them, as he had experienced in his own house. In the same number, Dr. D. Wooster publishes a report of his experience with the eucalyptus, which he has found especially useful in remittent and intermittent fevers.

Muscle as a Constituent of Nerve-Tissue. (*Gaz. Med. Ital.*, No. 52, 1871.)—Tigri has shown the presence of muscular fibres among the nerve-fibres of the cerebro-spinal nerves and the ganglia of the sympathetic. When an irritation is applied, contraction is caused, and the liquid contents of the nerve-fibres are caused to vibrate, and the motion is conveyed thus centripetally or centrifugally. In reference to these anatomical and physiological peculiarities, he calls the ganglia of the sympatheticus magnus, as well as the large ganglia of the brain, "nerve-hearts." He considers that the so-called Remak's nerve-fibres are muscular fibres.

Journalistic Changes.—The *Canada Medical Journal*, with the June number, completed its eighth volume, and calmly expired. It is succeeded by the *Canada Medical and Surgical Journal*, under the control of Dr. George E. Fenwick, and the *Canada Medical Record*, edited by Dr. Francis W. Campbell. These gentlemen were associated in the former journal.

Preliminary Medical Instruction.—A Board of Instructors has been organized in St. Paul, Minnesota, for the purpose of giving students an elementary course that will take the place of the office of practising now usually required. Such an introduction to the college courses is sorely needed by a large class of students.

Causes of Blindness.—Of five hundred cases at the Perkins Institution, South Boston, the causes of blindness were as follows: Congenital, 37.75 per cent.; due to disease, 47.09 per cent.; due to accidents, 15.16 per cent. The mortality among the blind in that institution was 8.9 per cent. greater than that of the general population of Massachusetts.—*Boston Medical and Surgical Journal.*

The Sensations in Poisoning by Strychnia.—The following letter, addressed to the editors of the *Medical and Surgical Reporter* of Philadelphia, appears in that journal for June 13, 1872:

The causes that led me to commit the rash act of which I am about to give an account can be of no particular interest; and, omitting all allusion to motives, and making no attempt at excuse, I will simply state the circumstances of the case, that I think may be of interest to those engaged in the advancement of medical science.

I am induced to do this by the request of my friend and physician, Dr. E. H. Coover, and the assurance that the case is a remarkable one, and likely to attract the attention of the medical profession everywhere.

On the morning of November 20, 1871, at about eleven o'clock, I swallowed *nearly*, if not *quite*, one grain of strychnia, which had been in my possession since the 16th of that month. I was sitting on the side of the bed, in the second-story back-room, in which apartment I have slept since I occupied the house. I took the powder dry from the paper, and held it in my mouth until it had become moistened with spittle, when I swallowed it, walked to the table and drank a glass of water, went into the room over the kitchen and hid the paper in which the poison had been, and then went down-stairs, where I took a seat in a rocking-chair in front of the stove. In the course of five minutes I began to feel slight cramps in the calves of my legs. My wife was sitting by the table sewing, and I asked her for a couple of crackers, which she gave me. The cramps had by this time increased in intensity, and extended to the feet and thighs, causing the most intense pain. I asked if there was an egg about the house, and, being answered in the affirmative, told my wife to break it and give it to me raw. She then suspected that I had taken something, and began questioning me closely about it. I expected soon to be in eternity, and thought that no human power could avail to save my life, so I told her what I had done. She immediately raised an alarm, and Mr. John Hoyer came in, followed by several other neighbors. The egg was given me, besides a large quantity of warm salt-water, and strong tea, and a messenger was sent for Dr. Coover.

About the time I called for the egg I attempted to rise from the chair, but fell to the floor with convulsions in the lower extremities, which became violent on the least attempt to move, and the feet were drawn in toward each other, becoming stiff and immovable save as the occasional convulsions shook them. Unsuccessful attempts were made to bathe them in hot water, each effort to raise me bringing on a violent paroxysm, in the last one of which I thought my jaws had become unhinged. I was now perfectly paralyzed from the hips down, and suffering the most exquisite pains, which began to extend upward, the muscles of the shoulders and neck soon being considerably convulsed, the forearms still being free from pain.

I now bade my wife "good-by," and prepared for the final struggle, which I knew must be near at hand, as I now had become rigid from the neck down (save the forearms); the convulsions of the muscles were becoming fearful, and the torture awful to endure. And now came on a tremendous convulsion. My hands were drawn in to my sides, with the fingers drawn apart and slightly bowed, and the jaws became rigid. I felt myself raised as if by some mighty power, and fixed immovably, with only my feet and head touching any thing. I heard some one say, "It is all over with him," and felt something like a black pall settling down upon my brain, when I became unconscious of every thing except my own agony, which was now beyond all description. I could feel my heart fluttering like a wounded bird, and my brain beating and throbbing with an irregular motion, as though at every beat it would burst from its confinement. Every joint was locked, and every drop of blood seemed stagnated. I remember thinking that it could not last long thus, when I must have lost consciousness.

I remember nothing more until I felt a sensation of relief, as though the garments of Death which had been drawn over me were being now drawn back. Those terrible cramps seemed to be descending toward my lower limbs; a sweet feeling of relief stole over me, and I began to be again conscious. Turning my head, of which I now had command, I saw Dr. Coover kneeling by my side, pouring medicine into my mouth from a spoon, and spoke to him, adding, "Here is the man that can save me," or words to that effect.

I was now free from cramps as far as the hips, and was fully conscious of all that was going on. I could feel the cramps receding before the antidote, and leaving the limbs free from pain until after the second dose administered by the doctor, from the time I resumed consciousness, when I was entirely free from cramp, with the exception of a little in the feet. I was then moved into an adjoining room, and placed upon a pallet, feeling comparatively comfortable. I had but one attack of cramps afterward, which was immediately relieved by a dose administered by my wife, the doctor having left for a short time; and when he returned I felt that the poison was completely neutralized.

I have since felt no return of cramps or convulsions: have had a good appetite, and could be out of bed were it not for the soreness in the muscles caused by the intense pain to which they were subjected.

Claude Bernard on Animal Heat.—M. Claude Bernard has only quite recently terminated his series of very interesting lectures on animal heat. The last course was devoted to the physiology of heat. The one next year will take in all the pathological bearings of the question, but, in his concluding lecture, a few days ago, M. Claude Bernard stopped to make a few general remarks on the pathological and physiological bearings of his subject as connected together. Among other things, he said that "health and disease were not, as formerly believed, two distinct and absolutely-contradictory conditions. They were only shades of the same physiological state. Health and disease did not fight for possession of the body. The same process still continued, only it was better regulated during health than during disease. Disease did not cause the appearance of new products; thus diabetes is marked by a more abundant production of sugar, but sugar was also produced in the healthy state." "Physico-chemical manifestations," he said, "do not change in character according as they take place within or without the organism; or, again, according to the condition of health or disease. There is but one kind of calorific agent, and it always remains identical with itself, whether it be produced in a focus or in the organism. There cannot exist a physical heat and an animal heat, nor a morbid heat and a physiological heat. Morbid heat and physiological heat differ only in degree, and not in kind."

The experiments which M. Bernard has performed on the circulation of the blood are of great interest. Among other facts and inferences the following may be briefly related: Circulation is accelerated or slackened according as such and such a nerve is operated upon. In one of the experiments the following facts were observed. A dog was submitted to the action of woorara. When quite motionless, respiration and circulation continuing, the extremity of one ear was cut off, and the quickness with which the drops of blood fell one after another served to note the speed of circulation. A glass tube was then introduced into the submaxillary gland so as to gather the secretion. The great sympathetic was then set bare in the neck, as also the sciatic nerve in the posterior region of the thigh. The cervical string of the sympathetic was then cut, and then the blood was seen to flow much more rapidly, the drops quickly succeeding each other. At the same time, a flow of saliva from the submaxillary gland into the tube was observed. Just then, however, on galvanizing the upper end of the great sympathetic, the blood diminished, and finally ceased running, through the contraction of the small arteries of the ear, the contraction sometimes going so far as completely to obliterate the arteries. These phenomena changed as galvanization of the sympathetic was suspended.

All the above phenomena, as shown by M. Claude Bernard, are localized modifications with which the central organ of circulation has nothing to do. This is not the case when the sciatic nerve is operated upon. In order to compare results, both sides of the animal may be experimented upon at the same time. Thus the left ear being cut off, the operations performed on the right side by galvanizing the sympathetic, and suspending the galvanization, have no influence over the flow of blood from the left ear. But, when the sciatic is operated upon, the flow of blood becomes more intense from both ears, while the same result obtains with the submaxillary glands. Thus there are reflex nervous phenomena which, through the dilatation of the vaso-motor nerves, increase the activity of the peripheric circulation, and these phenomena are produced more rapidly on exciting the sciatic nerve than on exciting the sympathetic.

M. Bernard has been led to recognize two antagonistic mechanisms which regulate the work of the capillary vessels, and indirectly that of the heart; they are the sympathetic forming a constricting mechanism, while the depressor nerve, and the sensitive nerves generally, suppress the constrictive effects as a consequence of their action. This double action, forming a complex mechanism which it is difficult to analyze, gives the clew to calorification, and consequently to fever and the morbid phenomena connected therewith. The whole circulatory apparatus would be thus subjected to a double spring, one of which stretches while the other does not, and health would be nothing else than perfect equilibrium between these two powers, constantly active and opposed to each other.—*Lancet*, July 13, 1872.

Calcutta Milk and Dairies.—Calcutta, or rather Chowringhee, was a few weeks back agitated upon the subject of milk. Cholera was believed to lurk in the cream-jug. Death stared you in the face out of custard-puddings. The agitation subsided, as Calcutta agitations always do, within the usual nine days. One or two energetic persons insisted on making the *gowala* bring his cow to the house, and are now happy in the belief that their milk is undiluted, forgetting to test the emptiness of the milkman's lotah before the lacteal streams descend therein. We are sorry to have to draw attention to a subject which may seem worn out, but the following description of one of the principal dairies in the place, as given by the Cattle-Plague Commission, is so very disagreeable, that it is a matter of public duty to make it known. The dairies consist of a number of huts, rather irregularly placed among several pools of dirty greenish-brown water called tanks. Some of these tanks are covered with vegetation, which has to be skimmed when any one goes to draw water to bathe. The water is actually thick with filth. The population of the surrounding huts bathe, wash clothes, and clean grain in these tanks, from which all the water for domestic purposes, and also for *diluting the milk*, is regularly drawn. A dairy-hut is a long, unpartitioned house with walls of bamboo matting, sometimes daubed with cow-dung and clay, and having a tiled roof. The general size is ten yards long, and four yards wide. The ridge-pole is not more than eight feet from the ground. One man has ninety cows in four of these sheds. Down the centre runs a single narrow drain about nine inches wide, and one foot deep. On each side of this the floor is planked, and outside the planks flagged. On the planks the cattle stand in two rows with their hind-feet close to the drain. There are no separate stalls. The cows are packed so close that they touch. The whole ventilation is one door at the side, and a small window at either end. Outside is a veranda in which straw is piled, and where cattle suffering from *rinderpest* are placed for segregation. The end windows, though meant for ventilation, are generally closed, and a *purdah* hangs in the door-way within which a smouldering dung-fire constantly burns to keep flies off the cattle. The centre-drain is open at each end, and falls into cess-pools which vary in size from six to twenty feet in diameter and are generally ten feet deep.

Their contents are never removed as a whole. The solid manure is made up into cakes as required for fuel—and the putrid liquid stuff is surreptitiously turned into the municipal drains—if any of these are conveniently near. Otherwise it is allowed to *fester* till a municipal prosecution compels the owner to fill up the pool. This is done by throwing earth and litter on the top. Vegetation follows, and an artificial quagmire is produced. So numerous are these that strangers to the neighborhood, to avoid accident, require a guide. Into one pool the commission threw a match, and instantly the surface was covered with flame. The cows are fed on oil-cake, which accounts for the oily taste of Calcutta milk. Is it to be wondered at that cattle kept under these insanitary conditions are constantly ailing, and frequently decimated by *rinderpest?* Is it to be wondered that cholera abounds in a town where *bustees* of this description are to be found even near the principal roads and streets? Can any thing excuse the apathy which year after year allows such nuisances to be and flourish? Every European and native justice ought to be compelled minutely to inspect the *bustees* under the conduct of the health-officer. On the occasion of this inspection they should be allowed no cheroots, no *pan*, no eau de cologne—and no one should be permitted to hold a pocket handkerchief to his nose. At the close of the ordeal, each justice might be allowed half a *seer* of carefully-diluted milk by way of refreshment, at the expense of the rate-payers. The visitation to be monthly, till the *bustees* are cleansed and reformed. What harmonious and vigorous action we should see if this were only tried!—*Indian Observer.*

A Shocking Scene in an English Court.—Americans are sometimes reproached with a lack of certain graces and refinements supposed to be peculiar to European civilization, but we venture to say that not even in the roughest or most primitive court of justice in this country would the scene described below have been possible. There is something astonishing in the utter lack of decency exhibited, to say nothing of the want of compassion for an unfortunate woman in the pangs of labor. The *Medical Times and Gazette*, from which we take the following, says complacently: "We suppose such a harrowing scene could hardly have been avoided." We should like to know why not. English courts must be strangely constituted if they cannot be adjourned for sufficient cause. But here is the story:

A scene was enacted in the Assize Court, at Chelmsford, on Monday last, which was probably unique in a court of justice; and certainly those who witnessed it are never likely to forget it. A young and, as the newspapers express it, "interesting-looking" married woman was arraigned for the wilful murder of her husband's first wife by poison. The deceased had died on October 10th last, and the prisoner, who was married only in December, was far advanced in pregnancy. The woman was in the dock, with a couple of brief intervals, for the space of seven hours, and bravely she bore the trying ordeal, conducting herself with as much ease and self-possession as any lady while dispensing the hospitalities of her own drawing-room. In the course of the afternoon, just after Sergeant Parry had cross-examined Dr. Stevenson relative to the presence of arsenic in the viscera of the deceased, and the Hon. G. Denman was commencing his reëxamination, the prisoner, who had risen from her seat during the delivery of Dr. Stevenson's evidence, was led out of court, and the rumor spread that her confinement was imminent. Her speedy reappearance, accompanied by Drs. Gilson, Finch, and Carter, reassured the court, and it was intimated to the judge that the trial had better proceed. It was evident, however, to skilled eyes that the poor creature suffered from either true or spurious labor-pains. A couch was brought, and the examination of the expert was resumed. For the next hour or two the scene was ghastly in the extreme. Labor-pains recurred at one time every three or five minutes, the prisoner alternately reclining, sitting, and standing; the evidence, meantime, proceeding as if nothing had happened. At one time two medical men were with her in the dock at the same time, and it was evident that their cheery counsel revived the poor creature. At 5.30 P. M., the evidence for the prosecution being finished, and no evidence being forthcoming for the defence, the question arose what was to be done. Baron Martin offered to go on with the case till midnight if necessary, but it was represented by counsel that the medical men were of opinion that the poor creature might be the better for a night's rest; so the trial was adjourned till next morning, and the excited spectators, among whom were a large number of unmarried females, dispersed. In the course of the evening the prisoner was delivered of a child; next morning the jury was discharged, and the trial postponed for three months. We suppose such a harrowing scene could scarcely have been avoided. What the consequences would have been had the trial proceeded, and either a verdict of guilty or of not guilty been pronounced while the woman was in the dire anguish of travail, we cannot say. Happily such another tragedy as this can rarely be enacted in all its details. Meanwhile, the miserable woman is condemned to a further three months of suspense.

The Tobacco Controversy.—Notwithstanding, says the *Lancet*, the counterblasts of kings in the past, and the tirades of the anti-tobaccoites in the present, people refuse to listen to those who pipe to a perverse generation from the lower rooms of Exeter Hall. Go where you will, at home or abroad, and more abroad than at home, people will smoke. From Napoleon and Bismarck down to the private soldier and laborer, they consume tobacco in some form or other. The members of the Anti-tobacco Society have a sorry time of it. If they have not striven to enlarge their minds by travel, they can scarcely have avoided an occasional railway journey. The establishment of a system of "smoking-carriages" on our lines of rail must have seemed to them a sign of our rapid national decay. The fumes of tobacco have, from their standpoint, assumed the proportions of the cloud of smoke that arose out of the fisherman's vase in the "Arabian Nights;" for the chairman of the society declared that "the practice of smoking hung like a gigantic cloud of evil over the country." And he went on to attribute so many terrible diseases to the same practice, that we almost wonder there are any smokers left to occupy the carriages set apart for them on railways. We are far from denying that tobacco-smoking has its evils; but this kind of exaggeration defeats itself. It is very unwise to seize upon a cheap pleasure and label it with a bad name. Common-sense individuals laugh at those who do so; and, what is worse, some people come to regard those who oppose practices that are really dangerous and degrading, as so many prigs giving to canting. Now, tobacco-smoking is no doubt injurious to the young, and we would counsel them not to commence it. To all constitutions it is hurtful when used in excess; and to many constitutions it is injurious in any quantity, however small. It is occasionally, no doubt, productive of dyspepsia, muscular tremors, and nervous palpitation, and it is apt to induce habits of listlessness and dreamy indolence; but, take it as a whole, tobacco-smoking is, nevertheless, to many people, an innocent gratification enough. Let us allow, that it is an unnecessary, often a selfish, and occasionally an injurious practice; but, to the poor man, working hard and living hard at the same time—to the soldier, fatigued, cold, and ill-fed during a campaign—tobacco is, we believe, both useful and comforting. It soothes some excitable men, and enables many another to concentrate his attention on subjects requiring thought. If the tobacco be the poison that its enemies declare it to be, it is eminently slow in its action, for every workhouse, lunatic asylum, and charitable institution, has its gray-haired votaries to the pipe.

The Vaccination Laws of Europe.—The *Medical Times and Gazette* gives the following interesting information regarding vaccination laws:

France.—Vaccination is not compulsory in France. There is no opposition to vaccination on the part of the people generally; but in some country districts there is a disinclination to have the children vaccinated. A certificate of successful vaccination is required before admitting a candidate to the Government schools, and also in some private schools, and also in some trades. In the present state of public opinion in France, it would be very difficult to enforce a compulsory vaccination.

Spain.—Vaccination is not compulsory in Spain, the Government limiting itself to recommending the practice. Vaccination is generally performed before the completion of the first year of infantile life, the most healthy period of the year being selected for its performance. There is no penalty imposed upon parents for the non-vaccination of children. Revaccination is not compulsory, but it is also recommended, and has been at different times practised in the army. The only civil disability attached to non-vaccination is that unvaccinated children are refused addmission into the public schools and some other establishments.

Belgium.—Vaccination is not compulsory in Belgium. There is no general opposition in Belgium to the practice of vaccination; still, the benefits of the practice are not so generally appreciated (especially by the lower classes of society) as to deserve any compliment upon the foresight of parents in taking care to have their children vaccinated.

Netherlands.—There is no compulsory vaccination-law in the Netherlands, and, of course, no penalty for its neglect. The vaccination of children is indirectly promoted by the local authorities by refusal of admission into public schools, and occasionally private schools; in the whole of the province of Friesland, vaccination is thus indirectly promoted. No legislative enactments on the subject of vaccination exist at present, but regulations on this head have been proposed in a bill at present before the States-General.

Frequency of Ovaritis.—Dr. J. Matthews Duncan, in the course of an article contributed to the *Edinburgh Medical Journal*, says that, although unable to give a good idea of the frequency of ovaritis, he is convinced that it is a more common disease than is generally supposed. He says that three forms have been described—1, parenchymatous; 2, follicular; 3, peritoneal—but that he is unable to see any real distinction between the parenchymatous and the follicular disease, and that the peritoneal form is out of the question, since the ovary has no peritoneal coat. He admits, however, that perioöphoritis, or inflammation of the adjacent peritoneal membrane, may occasionally occur. Ovaritis may be acute or chronic; the former may terminate in resolution, or its end may be complicated by paraphoöric adhesion or abscess, or true ovarian abscess, or it may end in the chronic form of the disease; the latter may last for several years. One ovary may be affected, or both, or the disease may affect the right and the left ovary alternately. The organ may be enlarged to the size of a hen's-egg. Ovaritis is only to be made out by physical examination. Palpation of the hypogastric region will reveal a feeling of fulness or tightness over the affected gland. The ovary may often be felt through the vagina, and, when inflamed, it is exquisitely tender, rounded, and more or less enlarged. When the ovary is bound down by adhesion, it may sometimes be difficult to distinguish it from the fundus of a retroflected uterus; but the distinction is generally possible if a uterine sound be used. Ovaritis is more frequently accompanied by too profuse menstruation than by amenorrhœa, is occasionally attended with leucorrhœa, and is not inconsistent with fertility. It is extremely frequent in the newly married, and in others it may be produced by excessive sexual intercourse. Dr. Duncan does not recommend any peculiar or special treatment.

The Pathology of Bright's Disease.—At a meeting of the Royal Medical and Chirurgical Society held May 28th, a paper was presented, prepared jointly by Sir William Gull, M. D., and Henry G. Sutton, M. D., "On the Pathology of the Morbid State commonly called Chronic Bright's Disease." The *British Medical Journal* says the conclusions arrived at in the paper were briefly as follows: 1. There is a diseased state characterized by hyalin-fibroid formation in the arterioles and capillaries. 2. This morbid change is attended with atrophy of the adjacent tissues. 3. It is probable that this morbid change commonly begins in the kidney, but there is evidence of its beginning primarily in other organs. 4. The contraction and atrophy of the kidney are but part and parcel of the general morbid change. 5. The kidneys may be but little, if at all, affected, while the morbid change is far advanced in other organs. 6. This morbid change in the arterioles and capillaries is the primary and essential condition of the morbid state called chronic Bright's disease with contracted kidney. 7. The clinical history varies according to the organs primarily and chiefly affected. In the present state of our knowledge, we cannot refer the vascular changes to an antecedent change in the blood due to defective renal excretion. 9. The kidneys may undergo extreme degenerative changes without being attended by the cardio-vascular and other lesions characteristic of the condition known as chronic Bright's disease. 10. The morbid state under discussion is allied with the conditions of old age, and its area may be said hypothetically to correspond to the *area vasculosa.* 11. The changes, though allied with senile alterations, are probably due to distinct causes not yet ascertained.

Cinchona-Cultivation in India.—The "Report on the Government Cinchona Plantation for 1870–'71" gives satisfactory accounts of the growth of the plants. From shrubs, the older plants of the several medical kinds have grown into trees 22 feet or 23 feet high, and 18 inches to 21 inches in girth. The *Cinchona succirubra,* in its finest specimens, reaches a height of 30 feet, with a girth of three feet. Of the Calisaya variety, 3,500 plants have been permanently planted out on two acres of new land. The Pitayo-bark, among the new species of plants lately introduced, seems hardy and suited to the climate. During the year 51,353 pounds of fresh bark were supplied to Mr. Broughton, the Government quinologist, for the manufacture of amorphous quinine; while 1,560 pounds have been extracted from 1,000 eight-year-old plants in the course of the year. A clear profit of one rupee per pound will, at the present rate of 2*s.* 8*d.* per two pounds, be yielded by this average of two and a half pounds to each tree. Increasing profits may be yearly expected, with a steady improvement in the quality of the bark; but in the home-market it is unlikely that India will ever compete in the cheaper kinds of bark with America, where the plant grows wild. In the finer kinds, however, a successful competition is far from improbable, if the mossing process be steadily applied. Under this process each successive renewal of bark becomes more valuable than the last, until red bark yields 12 per cent. of crystallizable alkaloids containing 8 per cent. of pure quinine; while 10 per cent. of the latter may be expected from the coarser barks, of which no such specimen is found in America.—*Lancet.*

Chloralum as a Disinfectant.—Prof. A. Fleck, in the *London Chemical Review*, has an able article on the nature and value of chloralum, of which he says in conclusion: The disinfecting and purifying powers of chloralum stand below those of alum, or sulphate of alumina and copperas (protosulphate of iron), which further recommend themselves by their much greater cheapness.

To sum up the argument concerning the value and composition of the preparation of chloralum: 1. The preparations of chloralum have nothing in common with the similarly-sounding chloral hydrate, and are, in point of fact, mixtures of chloride of aluminium. 2. The preparations of chloralum contain chlorine combinations of lead, copper, and arsenic, which renders their employment not free from danger, and which would render their employment, as a medicine or as an astringent for open or suppurating wounds, dangerous. 3. The price of the preparations of chloralum bears no relation either to their nature or their effect. Considering that the liquid chloralum yields a clear profit of at least 700 per cent., and the wadding 400 per cent., the limits of honest trading may be considered as overstepped. 4. The result of these experiments is that chloralum and the preparations made from the same must be classed among the worthless arcana, and in the interest of the public health, as well as in the material interests of the public, a most decided warning must be given against the purchase of the same.

Alarming Effects of Chloral in Small Doses.—A correspondent of the *Lancet* furnishes the following cases:

A middle-aged, tall man, recently an attendant at a lunatic asylum, had a boil on the buttock. On the 17th of April I lanced it, and at bedtime the man took part (containing nine grains) of a chloral draught. Shortly he became "stone-cold," his teeth were "fixed," and he stared wildly about. A cold perspiration flowed from him, wetting his pillow and sheets. Clothes in abundance were put on the bed, a fire was lit, and he took warm brandy-and-water; he then began to "feel circulation." The next morning when I saw him he looked pale and anxious.

An agricultural laborer's wife, aged sixty-eight, with black hair, and a goitre affecting the middle of neck, suffered from tumultuous and irregular action of heart, and from pyrosis. She could get little or no rest at night. On the 17th of May she took seven grains and a half of chloral at 9.30 P. M. At 10 she was asleep and slept for two hours. Then she awoke with a scream, jumped out of bed, and sat on the edge of it, semiconscious. She recovered in five minutes, and was got back to bed, where she lay quietly for an hour, and then fell asleep again. In two hours more she awoke with much epigastric pain.

The chloral used in both cases was of Liebreich's manufacture.

Ligature of the Subclavian.—Sir W. Fergusson recently ligatured the subclavian for aneurism of the third part of the artery. The patient, a man about forty years of age, had noticed a pulsating swelling at the root of the neck, on the left side, since Christmas. He had been under treatment, and was at last sent to the hospital. The tumor was near the mesial line, and it was difficult to decide, before operating, where the artery should be tied. An incision was made along the clavicle, and then others above and below at right angles to it. After a long and careful dissection the aneurismal tumor was come upon, overlapping the anterior scalenus muscle. It was pushed outward and downward, and then the scalenus was seen; there was then some delay in making out the artery; at last it was seen on the outer margin of the muscle, and was ligatured there. Sir William Fergusson said this was the fourth time he had ligatured the subclavian; the first time more than forty years ago. The operation was a troublesome affair, as most of them are. A curious thing was noticed toward the end of the operation—a white, serous fluid was seen at the bottom of the wound, and probably the thoracic duct was injured; yet it might not be so, as the subclavian was not seen, and he did not think he was near the angle where the duct joins the vein. The danger in operating on the left side is always greater on account of the duct.—*Lancet.*

Excision of the Knee for Gunshot-Wound; Recovery.—Excision of the knee-joint for gunshot-injury has proved so notoriously unsuccessful in military surgery, that authorities on the subject are generally disposed to condemn it. An exception to the general rule of fatality has, however, been recorded by Dr. E. Ritzmann, of Berlin, in the *Berliner Klin. Wochenschrift* for June 3d. The case was that of a Prussian soldier, aged twenty-four, who was shot through the right femur, close above the knee, at the battle of Gravelotte, on August 18, 1870, and on October 13th was brought to Berlin in an ambulance-train, under the guidance of Prof. Virchow. Considerable suppuration set in, attended with severe remittent fever; and, on November 9th, the joint was excised by Dr. König. The limb was put up in plaster-of-Paris, the joint being left exposed, and a drainage-tube being inserted to allow the escape of pus. The progress of the case after the operation was remarkably favorable, there being only temporary febrile disturbance in December and January; and, when the patient was discharged on May 23, 1871, there was complete anchylosis of the limb in a slightly-bent position, with shortening to the extent of two and a third inches. On September 26th the patient gave a favorable account of his condition.—*British Medical Journal.*

Craniotomy under Difficulties.—The Melbourne correspondent of the *Medical Times and Gazette* gives the details of a peculiar case: A medical man named Jackson, residing in one of the thinly-populated districts in the West, where a practitioner has to ride probably thirty or forty miles to see half-a-dozen patients, attended a case of midwifery March last, and had to perform craniotomy. There was nothing remarkable in the case, beyond the fact that the head had become impacted, turning was not practicable, and the woman was rapidly becoming exhausted. But, being many miles from his residence, and having no instruments with him, he did the best he could with such instruments as he could extemporize. With a chisel, a gimlet, an auger, and a hook made out of a piece of telegraph-wire, he delivered the woman successfully, and she recovered without the least bad effect after. But two rival practitioners of base-minded proclivities actually set on the woman's husband to move the coroner to hold an inquest on the remains of the child, and so worked upon the prejudices of the people in the district that the jury returned a verdict of manslaughter against Mr. Jackson, and he has been committed for trial, notwithstanding the most favorable representations on the part of the medical society of which he is a member.

Health of the City of London.—The twenty-third annual report of the Medical Officer of Health for the City of London has just been published, and is, in many respects, exceptionally interesting. The population of the city (as taken at midnight on a Sunday last year) amounted to 75,842 persons. Its area comprises 723 statute acres, and there are 9,435 inhabited houses, 8 persons to a house, and 105 to an acre. The day-population of the city is estimated at more than a quarter of a million, so that the density of the population, both by day and night, is still excessive, although the number of residents has decreased since 1841 about 80 per cent. The death-rate is at the rate of 23, and the birth-rate 24.2 per 1,000, so that, comparatively speaking, the former is not excessive and the latter is evidently low. Small-pox killed 88 persons in the city last year, as compared with 8 in 1865, 29 in 1866, 31 in 1867, 18 in 1868, 2 in 1869, and 6 in 1870; and, in discussing the prophylactic advantages of vaccination, it is remarked, that in Christ's Hospital, which has an average of 800 boys, and where it is the practice to vaccinate all on entry, one death only has occurred from small-pox during the past fifty years.—*Lancet.*

Ammonia in Suspended Animation.—The value of the injection of ammonia, as recommended by Prof. Halford, in cases of snake-bite and suspended animation, has been again demonstrated. A lady in Melbourne recently swallowed by accident an ounce of Browne's chlorodyne, which is a mixture of chloroform, morphia, and prussic acid. When seen by her medical attendant, she was, as he imagined, on the point of death—cold, insensible to every thing, and giving only occasional gasps as signs of breathing. Recollecting a former case in which a young man who had taken chloroform was revived after death had apparently occurred, the doctor mixed half a drachm ot the liq. ammon. fort. with one and a half of water, and within the space of one minute injected the whole into a vein of the arm. In a few minutes the pulse returned, the breathing became natural, and by twenty minutes the whole body had regained its natural warmth; but perfect consciousness did not return for some hours afterward. The patient made a rapid recovery. Two further instances have also been reported in which the timely use of the injection saved the victims of snake-bite from the death which threatened them.—*Melbourne Argus.*

New Plan of extracting Bodies from the Ear.—Dr. Loewenberg, of Paris, describes a new plan for extracting solid bodies from the ear, as follows: A very small brush is made by rolling and fixing a narrow strip of old linen around a thin wooden handle (a match, for instance), and unravelling its free border to the length of a quarter of an inch. The end of the so-obtained fringe is dipped into a warm and very concentrated solution of glue, applied to the visible part of the foreign body—or rather the operator leans it against the body by letting it glide very softly, and without exercising any pressure, over it. Previous to the application the patient seats himself comfortably in an arm-chair or on a sofa, and inclines his head toward the healthy ear. He remains in this posture for three-quarters of an hour to an hour after the introduction of the agglutinated brush. This time past, consolidation is generally accomplished, and the foreign body can be extracted by gentle pulling at the brush.—*Medical Times and Gazette.*

Death from the Sting of a Bee.—On the 19th instant, Miss Elizabeth Hough, of Twickenham, aged fifty-five, was stung by a bee behind the ear. Shortly afterward she became unconscious, and died before the doctor who was sent for arrived. The brother of the deceased stated at the inquest that she was of a delicate and nervous constitution. In the autumn of 1870 she was stung by a bee, and the consequences were very peculiar. She became unconscious, and remained in that state for two hours. It is well known that many people endure far greater sufferings than others when stung by bees or other insects, and cases, in which serious consequences have ensued, are recorded. Beck, in his work on "Medical Jurisprudence," quotes a case in which a sting on the back of the finger was quickly followed by "vomiting, sweating, trembling, and great difficulty of breathing."—*Lancet.*

A Triumph for Lady-Doctors.—The *Medical Times and Gazette* of May 11th says: "Our advertising columns to-day contain a novel announcement. The Birmingham and Midland Hospital for Women wants a resident medical officer possessing a medical degree or diploma granted after due examination. Lady-doctors are admissible as candidates. This is the first appointment of the kind which has been opened to ladies in this country."

Vaccination and Revaccination.—The value of vaccination and revaccination is becoming generally appreciated. Marshal MacMahon has given orders for the entire army of Paris to be vaccinated or revaccinated. A measure has been submitted to the Chilian Congress to make vaccination compulsory.

Obituary.

DR. BOUSQUET, the most eminent of the promoters of the Jennerian doctrines in France, has just died, aged seventy-six, having long since retired from practice with a good competence, which he has devoted to the erection of and providing for a hospital. In medical history he will be remembered as one of the most able and determined opponents of Broussais in the pages of the *Revue Médicale*, between 1820 and 1830, and therefore when the great antiphlogistic advocate was at the height of his popularity. Bousquet's "Traité sur la Vaccine" is one of the best works upon the subject, and has done immense service to the cause he had so much at heart, and which he defended to the last when it was threatened by the inroads of "animal vaccination."—*Medical Times and Gazette.*

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ART. I.—*Astronomical Etiology.* By M. L. KNAPP, M.D., Cadereyta, Nuevo Leon, Mexico.

WHEN astronomy came up, astrology went down. The cause of the going down of judicial astrology was the coming in of astronomy as a science. In all periods of the world's history there has existed a belief in the influence of the stars on the earth and its inhabitants. During the dark ages, this astrological superstitious belief was an intolerable scourge. So late as the latter part of the twelfth century, the Bishop of Canterbury ordered a solemn fast to be observed of three-days' continuance, on account of an unusual conjunction of planets and calamities predicted by astrologists.[1] No extraordinary event happened, however, in England during the year, but in the following year epidemics and epizoötics occurred. The difference between astronomy and astrology stands about thus: astronomy is the *law* or *rule* of the *astra.*[2] By the term *astra* the ancients understood all the constituents of the heavens—sun, moon, and stars. Astrology had much the same meaning originally, but, for lack of science, it sank into a degraded system of fortune-telling, and the predicting of future events by the aspects and surmised influence of the planets. Astronomy has a clean record. It has never bowed down the knee to Mammon. The extraordinary advances now being made in the science of astronomy, and the conclusion finally arrived at of a resisting medium filling all space, justify the present inquiry into the question of the astronomical etiology of general pestilence. If the science of astronomy teaches that the solid planets circulate in a fluid elastic medium, common to all, why should not the inference be drawn that all must feel each other's movements, vibrations, impulses, and forces, as they play around each other in this elastic medium? The inference is just—it is so admitted.

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[1] "History of Pestilence," by Noah Webster, vol. i., p. 125.

[2] "Outlines of Astronomy," by Sir John Herschel, page 24.

Modern astronomy teaches that all the members of the solar system are kept in their elliptic motion around the sun by a force of matter called gravitation; that the sun, which is a body immensely more massive than would be another composed of all the other members of the system if collected into one, and occupying the centre, is the centre of activity—of light, of heat, of electricity, of magnetism, of gravity, of motion—by which it verily rules all the minor circulating-orbs that surround it, gathering them and reciprocally receiving their return forces. How this is effected, and whether these supposed several forces are essentially different, or only one and the same, under different modes of manifestation, we have not yet ascertained. Astronomy has made rapid strides since the days of Kepler and Galileo, and it is not impossible, perhaps not improbable, that scientists may yet unfold clearly and satisfactorily the laws by which this self-acting, stupendous, thermo-gravito-electro-magnetic machine, the solar system, moves, is kept in motion, and the value of the dynamical power or influence each member of the solar system may exert on each of the others in maintaining, as well as impairing, the conditions on which animal and vegetable life thereon depend. The study of this problem commends itself with great force to the attention of physicians as a fertile field of research. While astronomers are intent and eager in the line of discovery, physicians, though they may be only amateurs in astronomical science, are peculiarly fitted for making researches in astronomical etiology. The divine art beckons them into the celestial fields of research, when terrestrial sources fail to give satisfactory evidence of the cause of general pestilence. The anxious professors of this divine art have labored hard, but in reality in vain, for two thousand years, among the fogs and fens of earth, for the cause; have explored diligently the air, the water, terrene emanations, the forces of catalysis, zymosis, and eremacausis, and have, in these latter days, almost believed (very unphilosophically) they have found it in the *ignis-fatuus* hypotheses of marsh-malaria, animalculæ, and contagion; in fine, they have projected hypotheses, launched forth theories, discoursed sagely on occult qualities, and explored faithfully all the avenues and sources of disease this world of ours contains, and are as far removed now, in this day, as they were in the days of Hippocrates, the father of medicine, from a true and satisfactory explanation of the cause of epidemics. Some have long since given up the search in despair, declaring no cause could be assigned but the will of God (Procopius). Others have rashly ascribed their frequent and awful visitations to the vengeance of God for our sins (Diemerbroek). Others, when earth gave no answer, have ascribed their coming to the influence of the stars (the ancients); but astrologically, superstitiously, and blindly as to *how* the influence comes—a theory too unscientific to be listened to for a moment. And it has been held, too, that the coming of comets was the cause of the coming of epidemics (Webster). But, were this true, we should have epidemics every year.

It is hardly necessary to say that rigid science has accepted none of these imaginings. Neither the air, water, terrene emanations, chemical forces, occult qualities, malaria, animalculæ, astral nor cometary influences, nor yet any of earth's forces, have hitherto satisfactorily solved the vexed question of the coming again and again of epidemics.[1]

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[1] "Epidemic diseases," says a recent English periodical, "are generated, according to the most modern physiological doctrine, by a specific poison introduced into the body from without. . . . The poisons of various epidemic diseases are distinct. . . . We are able to note remarkable *waves of disease*. . . . In England we have recently had a succession of epidemics—cholera

Casting about for a point of departure, we know the *sun* is the origin, source, and fountain of life and health to all organized existences on the face of the earth; and we know that the intensity of its action is often such as to destroy the life and health of organized existences, both animal and vegetable, on the earth. We may reasonably infer the same benign and malign effects of the sun's action upon organized life on the other planets. No one questions the power of the sun for good and evil over organized life on the earth, nor probably over animal and vegetable life on all the members of the system designed for its manifestation and maintenance. Besides its calorific, luminiferous, electric, and magnetic influences, we know it has an attractive influence on the earth; we see it raises an enormous tidal wave on the ocean. And this force is unlike any other attraction we know of, except mechanical, such as suction or lifting. It is not of the nature of the elective attraction of the chemist, nor of magnetic attraction, which is powerless on other substances than iron and some one or two more, but is of a more universal character, extending to all the material constituents of our system (Herschel). Wherever matter is, this force attends it. And we are taught to regard this force as the cause of the elliptic motion of the planets around the sun. We know this force becomes greater upon the earth the nearer the earth approaches to the sun. We know it by its increase of power in raising a tidal wave on the ocean, as the earth makes its perihelion circuit. But this force has not hitherto been considered an agent working either good or evil on the life and health of organized existences on the earth. The *heat* of the sun is considered a cause of epidemics; pestilence was ascribed to it by the ancients under the allegorical name of Apollo (Homer); but neither ancients nor moderns have ever ascribed pestilence to

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in 1866 was followed by typhoid fever, and, as the latter began to abate, scarlatina appeared in the most malignant form, . . . a little later relapsing fever. . . . And lastly we have been visited with an epidemic of small-pox more severe than any outbreak of that disease which has been recorded in England during the present generation. And no sooner has the small-pox begun to abate, than we are threatened with a return of cholera. This periodicity of diseases is yet to be explained."

extreme impressions of the force of gravitation. We see abundant reason, then, in the action of the *sun* on the earth (to say nothing of any other *aster*) for the recognition of astronomical etiology.

We know the moon also exerts an attractive force on the earth, and we are familiar with the popular belief of its power over the animal and vegetable economies and processes. Owing to its greater proximity, though a comparatively insignificant orb, it raises a higher tidal wave on the ocean than the sun. By this we know the force of gravity diminishes as the square of the distance increases, just in the same manner as the forces of light, heat, electricity, and magnetic attractions and repulsions, are enfeebled by distance. Be the essence of this force, then, what it may, it is a *real power* as much as light, or heat, or electricity, or magnetism; and its extreme impressions on organized existences on the earth are fatal to animal and vegetable life. Were this force to be suddenly augmented on the surface of our earth to the degree geometers calculate it for the surface of the sun, no human being on the face of the globe could survive the shock; the flues of respiration would instantly collapse, from external pressure upon the thorax; no one could stand up or sustain the weight of his own person. As has been quaintly said, man would be literally crushed to atoms under the load. A man of one hundred and sixty-eight pounds' weight would weigh four thousand six hundred and eighty-seven pounds on the sun (Herschel), a load no man could sustain for a moment. Minor degrees of an augmented impression of the force of gravitation, especially if long continued, and also diminished impressions and vicissitudes of it, differing from the degree to which, as a general rule, we are accustomed, and by nature adapted and conditioned, must also be more or less injurious, in a similar manner as excessive and defective impressions and vicissitudes of heat, light, and the other natural vital stimulants, are hurtful. In fine, we must, whether we will it or not, enroll this occult force in Nature among the natural vital stimuli. No one surely could live *in vacuo;* he would bleed at every pore, even could the function of respiration be provided for; and very high altitudes above the level of the sea are productive of great disturbances to the harmony and well-being of animal and vegetable life, even far below the limits of perpetual snow. Nobody can breathe in comfort there, for lack of gravity in the atmosphere. Decarbonization of the blood cannot be healthfully carried on there, for lack of a sufficient supply of oxygen in the air. The conditions that render the earth habitable for the human race are subverted there, by lack of gravity to compress the atmosphere. The same in regard to the vegetable kingdom—none but stunted Alpine shrubs can grow there. At the level of the ocean all organizations are subjected to a pressure, by the gravity of the atmosphere, equal to about fifteen pounds to the square inch, and any considerable deviations from this degree, whether above or below, are hurtful. The dwellers in Alpine gorges are subjects of goitre and cretinism; they become dwarfed and deformed of body, and imbecile and idiotic in mind, and the why and wherefore are unknown to the science of etiology. What more rational explanation need we than to ascribe these effects to the lack of the vital stimulus of the force of gravitation long continued?[1]

Reasoning my way along in this manner, in untrodden paths of etiological investigation, and reaching out for a philosophic, scientific, and satisfactory explanation of the laws of epidemics (a study with me for many years), I have come at last to regard the periodical increase of planetary attraction that occurs when the superior planets make their perihelion circuits as the cause, *direct* and *indirect*, of the inauguration of epidemics, or recurrence of what are called pestilential periods. This postulate I am now to urge and advocate, with such arguments as have appeared to me to be scientific and tenable.

If the sun and moon disturb earth's ocean and atmosphere, by their force of attraction, elevating immense tidal waves thereon, with increase of gravity in the atmosphere appreciable by the barometer (Maury); if every particle of matter in the universe attracts every other particle, with a force directly proportioned to the mass of the attracting particle, and inversely to the square of the distance between them (Newton); and if the mass of Jupiter is by far the most influential element in the planetary system after that of the sun (Sir John Herschel); and Saturn the next, by general authority—surely Jupiter and Saturn's periodical approaches, in making their perihelion circuits, must affect the earth and the organized existences on its surface to a considerable extent or degree, by their increased attraction of gravitation, and disturbance of its atmosphere, and the natural vital stimulants of all organized life. And the same is philosophically true of the approaches of all the other planets in proportion to their masses and proximity. When all approach coincidently, the combined effect appears, by the illustrations which I will offer, to be very disastrous to the well-being of man, animals, and the crops and fruits of the earth. The precise manner in which the malign influence is effected is too occult to be formularized as yet. But, if we can know the cause of the disastrous effect as a general and scientific truth, it is a great advance made in etiology.

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[1] "In a memoir by Bert on the influence exercised upon vital phenomena by variations in barometrical pressure, it is stated that, if the atmospheric pressure to which a warm-blooded vertebrate is exposed be suddenly reduced to fifteen or eighteen centimetres of the barometric scale, the animal jumps about convulsively, is attacked with cramps, and dies very quickly, with bloody foam in the bronchia."—(See *Harper's Monthly* for February, 1872—"Editor's Scientific Record.")

Blights in vegetation invariably precede and accompany epidemics, and not unfrequently culminate in famines. But what is the cause of the blights? The coincidence of blights in vegetation and epidemics has been observed in all time during the historic period, but no one has answered the question, What causes the blights? The unsatisfactory answer, I was forced with regret to give this question in my "Primary Pathology and Origin and Laws of Epidemics," published in 1857, was, "*God only knows.*" But further time and researches have enabled me to answer this question, and, as I believe, in a philosophical manner. It is clear to me now, though I may fail to convince others, that the recurrence of the blighting years and meteorological inharmonies that cause famines and epidemics are chargeable to *planetary* influences.

I charge the malign cosmical influences of continued bad weather, and extraordinary vicissitudes, excessive cold, excessive heat, excessive droughts, and other malign meteorological inharmonies that destroy the crops and fruits, and inaugurate blights, famines, and epidemics, to *periodical excess of planetary attraction.* But I totally ignore astral influence in the degraded sense in which the ancient system of judicial astrology held it. The blights in vegetation, from bad seasons and irregular weather, always precede epidemics. True, the bad seasons and years often continue after the epidemics appear, and it is not unphilosophic to hold that planetary attraction may *directly* impair the nutritive life of man and animals as well as vegetables. The *position* of the planets is a recognized cause of the disturbance of terrestrial magnetism by men of science (Pliny Earl, Chase). We may not be able to explain why vegetables and fruits fail during certain years, yet we see this is sometimes the case; and, when to all appearances the weather is favorable, corn is blasted on the most fertile plains, and fruits in orchards and gardens fail to arrive at their usual state of perfection (Webster). A long-continued excess or deficiency of the vital stimulus of gravity may injure the delicate processes of florification and fructification by disturbing the other imponderable forces, and especially the *thermal*, *magnetic*, and *electric* conditions most favorable. It is a reasonable hypothesis. There are no data. We may not, in the present state of our knowledge, be able to explain the precise manner in which the increased force of the attraction of gravitation, under the perihelion approaches of the large superior planets, so changes the meteorological conditions or surroundings of our earth as to produce foul weather, unfavorable for crops and fruits, and also for animal life and health; and yet it is not unphilosophic to take such a view, knowing as we do that the force of gravity plays as important a part in the universe as heat or light; and seeing, as I shall soon illustrate, that the *pestilential periods are always coincident with the perihelia of the large superior plants, especially of Jupiter and Saturn.* Indeed, it would be unphilosophic to ignore the facts, and refuse to take such a view. The simplest manner in which it seems to me explicable, is, to consider attraction of gravitation as a natural vital stimulant, the same as heat, and the other imponderable natural vital stimuli, that a mean normal amount is necessary and salutary, and that extreme impressions and vicissitudes are hurtful. This may not be entirely satisfactory, and yet it is not an unphilosophic explanation. I cannot explain precisely how our accustomed amount of light operates on the organisms of the animal and vegetable world to maintain their well-being, but I know it does so operate. I see vegetables are sickly, grown in the shade, and etiolated grown in darkness. The temperate application of all the vital stimuli seems to be the *rule* favorable to life, or, in other words, moderate impressions are favorable to life and health, and extreme impressions injurious. Now, gravity being admitted to be a vital stimulant (we cannot live without it), the law of its action becomes intelligible, as intelligible as that of light or heat. And, although it appears impossible to construct a system of worlds operated by these forces, where their impressions shall be uniform and moderate, still it is not the part of wisdom in us to quarrel with the conditions under which we are placed; to complain that the vital stimulants are sometimes too forcible, and again too weak. We live by them and we die by them. But what if our lot were cast on the planet Mercury, under a sevenfold greater impression of solar heat and light than we are allotted here? Or on Uranus, under a three-hundred-fold less impression? Or on Jupiter, where we could hardly lift our feet to walk about, so oppressed would we be by the force of gravity? Or on an asteroid, where we could leap sixty feet high?

Great, indeed, must be the diversity of conditions to which organized life may be adapted, if we are to suppose all the planets covered with a teeming verdure, and swarming with animal life, with development up (by natural selection or otherwise) to the standard of earth's human intelligences—and this is but a rational view. Each inhabitant is adapted, of course, to his native conditions—to the degree of impression of the forces of his sphere or planet, and is as eager, doubtless, in the pursuit of happiness and knowledge as we of earth; and doubtless finds the vicissitudes of his climate and oscillations of the imponderable forces and atmosphere in which he lives and breathes as hurtful to his well-being as we of earth find ours—he lives by these forces and he dies by them. But this is speculation. Our business as medical philosophers is, to take note of the *conditions* under which light, heat, electricity, magnetism, and gravity, are found to be most favorable to life and health on the earth, and to observe, if we can, the disturbing forces that sometimes seem almost to subvert the conditions favorable to life and health. If we find these unfavorable periods *invariably* to coincide with the perihelion approaches of the large superior planets, we must concede their approaches to be injurious, even if we cannot, in the present state of our knowledge, explain the reason why. It may require a long series of observations and greatly-improved philosophical instruments to settle this question beyond cavil; but, looking at the progress made within the last few years in spectroscopy, we may reasonably indulge the hope that astronomical science will yet unfold something positive on this question to cheer the medical philosopher; some instrument more delicate even than the barometer, capable of measuring the amount of planetary attraction on the air and on the imponderable elements or forces. The barometer shows that there is an increase in the weight of the atmosphere at every high tide; elevating an immense atmospheric tide and counter-tide; and that there is then a marked change in the electric tension, and a variation in the magnetic needle. But science has not revealed whether or not these disturbances are augmented under the perihelion approximations of the large superior planets, coincident with bad weather, blights, and epidemics. I infer they are considerably augmented, and that their long continuance is detrimental to organized life on the earth. We are accustomed and conditioned to the annual forces of the sun, and monthly forces of the moon, and their changes. The hardy germs resist and live, but the weakly perish under their extreme forces. When the large superior planets are in their aphelion circuits, far removed from the earth, we get on pretty well—there is comparatively but little pestilence — but under their perihelion approximations organized life struggles and succumbs. I will illustrate by examples.

EXAMPLES OF PERIHELIA AND PESTILENCE.

The principles of astronomy set forth that, were there only one planet in our solar system (say the earth), it would revolve in an exact ellipse around the sun (or both around their common centre of gravity), and would so continue to revolve in the same orbit forever; that a second planet added (say Jupiter) would draw both the former out of their mutual orbits, and so disturb their relations as to put an end to the mathematical exactness of their elliptic motions; that any variation it might cause in the eccentricity of the disturbed planet's orbit would alter materially the conditions of existence of living beings on it, by altering the mean temperature of its surface; that an increase or diminution of its mean distance to a tenth part would totally *subvert* the conditions on which all life on the disturbed planet depends (Sir John Herschel); and a thousand and one other evidences that might as easily be gathered, going to show the perturbing influence of planetary attraction on the earth.

Now, setting out with this positive knowledge of the disturbing planetary forces acting on the earth, I hold it to be a not unphilosophic conclusion that the malign cosmical phenomena which invariably accompany and seem to inaugurate epidemics come of the augmented disturbance of the earth's atmosphere; and the imponderable forces of light, heat, electricity, magnetism, and gravity—the great ocean of meteorological vital stimuli in which we live and move and have our being—must then be very great, and cannot be harmless of evil. The revolutions of Jupiter, the most disturbing element in the system, seem to govern the recurrence of the pestilential periods. His period of revolution is eleven years and about three hundred and fifteen days—somewhat less than twelve years—and this interval of time corresponds most remarkably to the interval of recurrence of the pestilential periods.

The article on cholera in the August number of this JOURNAL for 1871, asserts that "cholera has broken out there" (Hurdwâr, India) "every twelfth year since 1783, and notably in 1867." Now, the perihelion epochs of Jupiter tally with this exactly. Calculating backward, Jupiter made his perihelion passage in 1868, 1856, 1845, 1833, 1821, 1809, 1797, and in 1785; and these were pestilential periods, not only in Hurdwar, India, but all over the world. Not only cholera, but all other forms of epidemic pestilence, raged in those periods. Jupiter made his perihelion passage also in 1773, 1762, 1750, 1738, 1726, 1714, 1702, 1690, 1679, 1667, 1655, 1643, 1631, 1619, 1607, 1596, 1584, and 1572; and all these periods were years of aggravated pestilence also, all over the world.

This is as far back as I propose carrying the examples in this paper, through three hundred years of the most historic period of the world; a sufficient length of time to test the truth or fallacy of the hypothesis. The reader should bear in mind that Jupiter is about six years in making his perihelion circuit, and that when passing his nodes his influence is probably as great, or nearly as great, on the earth as when in the technical longitude of his perihelion—longitude 11°—particularly when passing his descending node with the earth in the perihelion circuit, or higher apsis of its orbit, as in summer, the nearest approximation to each other the two planets ever make. Two or three years before or after Jupiter's perihelion epochs, therefore, are often very pestilential years, and may be legitimately charged to Jupiter's approaches; and some countries suffer sooner than others.

Saturn made his perihelion passage in 1856, 1826, 1797, 1867, 1738, 1708, 1679, 1650, and in 1620; his period of revolution around the sun being twenty-nine years and about one hundred and sixty-seven days. There being a near approach to commensurability in the mean motions of these two planets (five periods of Jupiter corresponding very nearly to two of Saturn), it so turns out that every fifth perihelion epoch of Jupiter occurs in the same year with every second perihelion epoch of Saturn, or within the space of twelve months, if not in the same year, during the whole period of the three hundred years proposed for analysis. And these commensurate periods, I find, without exception, to be *remarkably aggravated pestilential periods*, extending generally through a whole revolution of Jupiter, or even to fifteen years' or more duration. These periods will constitute my examples. They are bold and unmistakably pronounced. They occur in cycles of about fifty-nine years—fifty-nine years less sixteen days. Sir John Herschel says, in his "Outlines," page 400: "A cycle composed of five periods of Jupiter and two of Saturn, although it does not *exactly* bring about the same configuration, does so pretty nearly. Five periods of Jupiter are 21,663 days, and two periods of Saturn, 21,519 days. The difference is only 146 days, in which Jupiter describes, on an average, 12°, and Saturn about 5°; so that after the lapse of the former interval they will only be 7° from a conjunction in the same parts of their orbits as before." Five of these commensurate periods have occurred within the last three hundred years.

EXAMPLE I.—Such a period was that of the Irish famine, the last and best remembered, and which I will take as the first example, and calculate the series backward. It commenced in 1842 and extended to 1858. The two planets made their commensurate perihclia in 1856, Saturn in March and Jupiter in December. The five periods of Jupiter were 1856, 1845, 1833, 1821, and 1809. Most of my readers will recollect the yellow-fever epidemics of 1853 in New Orleans, 1855 in Norfolk, and 1856 in New Orleans again, with greater severity; and the ship-fever epidemics of the Irish-famine years, from 1845 to 1849, when they culminated in cholera; as well as the general pestilence, all over the world, of this period, extended through to 1856. And, glancing still farther backward, some of my readers will recollect the first outbreak of the Asiatic cholera in America in 1832 and 1833, under Jupiter's perihelion approach of that period. And some will recollect even the epidemic times of 1821 and 1809, also under Jupiter's perihelion approaches. All of these sickly periods are perfectly well remembered by me, and also the foul years coincident with and antecedent thereto. It is not necessary to dwell on the particulars of this well-remembered period of the Irish-famine pestilence continued through to 1856 under the commensurate perihelia of Jupiter and Saturn. It will be further illustrated, however, before closing.

EXAMPLE II.—Pursuing our calculation backward, we now pass into the confines of the eighteenth century, out of the memory of nearly all of the profession, to 1797, the next commensurate perihelion period of Jupiter and Saturn calculated thus backward; but fortunately we have Noah Webster's "History of Pestilence," written in that very period, published in November, 1799, called into being by the desire of that good man to serve his kind by searching after the cause of pestilence. He did not find it,[1] but his record of the recurring pestilential periods, through all the historic ages of the world, enables me to establish the coincidence of epidemics with the perihelia of Jupiter—and of Jupiter and Saturn—beyond all doubt. Jupiter and Saturn, I say, made their commensurate perihelia in 1797; Saturn in April, and Jupiter in August, according to my calculations; and another fact in point of great importance, Uranus made his perihelion passage the next year, 1798, in the month of August. And, according to Webster, the yellow fever was epidemic in every sea-port of the United States that summer, even as far north as Portland, in the State of Maine! Every physician, however, knows all about the yellow-fever epidemics from 1793 to 1798, when the American Hippocrates, Dr. Rush, so nobly contended with the foe; but every one has not learned to associate this fact therewith, that Jupiter, Saturn, and Uranus, the three largest planets of the solar system, were conjointly in perihelion then. This is a new etiological fact for epidemiological societies to consider.

Whoever will consult Webster's history, referred to, will find that this epidemical series of years was really carried, or driven through, from Jupiter's previous period, 1785, as the pestilence was driven through from 1845 to 1856. Jupiter's periods in this cycle were 1797, 1785, 1773, 1762, and 1750, all of which were marked pestilential periods, *according to Webster;* but, as I study brevity, I cannot dwell on the details of each of Jupiter's periods. Suffice it to say that this commensurate period of Jupiter and Saturn was more withering, scathing, and severely mortal, than our first example, the Irish-famine period, driven through to 1856. To cite one example of mortality from Webster: "I learn from the consul of Great Britain, at Alexandria, that it was calculated a million of people had perished by the plague in Upper and Lower Egypt, between 1791 and 1796; and our accounts from that country mention that the disease had not ceased the last spring" (spring of 1799).

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[1] "With respect to the primary causes of epidemic diseases," says Webster, "we are in the dark; but we are certain, from all history and modern observations, that the causes of epidemic diseases among the human race affect every species of animal and vegetable life."—(*History of Pestilence.*)

Thus we find the last two commensurate perihelion periods of Jupiter and Saturn to be almost fac-similes of each other. Have the planets any thing to do in the matter? But again:

EXAMPLE III.—The two planets, Jupiter and Saturn, made their commensurate perihelia in 1738, in the month of May, Jupiter about two weeks earlier than Saturn, the nearest approach to a synchronous adjustment that had occurred, or will occur again, for many centuries. Jupiter's periods in that cycle were 1738, 1726, 1714, 1702, and 1690; all of which were remarkably pestilential periods, as the reader will see by consulting Webster. I study brevity, and therefore do not detail the malign cosmical phenomena of each of Jupiter's periods, but cite the reader to the work where they may be seen. He will see, by consulting Webster, that the *whole cycle* was very pestilential, with aggravations at every return of Jupiter; and presently I will venture an opinion or reason why this was the longest and most distressingly sickly and mortal period of the whole of the eighteenth century. That it was so is certain, all over the world. Those who doubt it may consult not only Webster, but the account of all pestilences in the world's history reported in the Irish-famine statistics, "Census of Ireland for 1851." Lind on Scurvy may also be consulted. His work appeared after the epidemics of this commensurate period of Jupiter and Saturn. He was moved to write by the examination of Lord Anson's journal. Lord Anson circumnavigated the world during this pestilential period, and lost three-fourths of his crews by scurvy.

And now for the reasons why this was a longer and severer pestilential period than the Irish-famine culmination, or than even the culmination of pestilence of 1797, in Dr. Rush's day. Could it have been because there was more marsh-malaria generated in the swamps of the universal earth? Was it because the Almighty, in the secret councils of His wisdom, had commissioned the destroying angel to sow broadcast throughout the world's atmosphere certain inscrutable "*occult qualities*," "*malignant poisonous germs*," or other deadly principle, which no microscope or chemical test might reveal? Was some universal "contagion" the origin and cause of the world-wide scurvies of those years? No, indeed, certainly not; it was not owing to any one of those puerile hypotheses. It was owing rather to the great and adequate *astronomical* cause, the commensurate perihelion of Uranus and Neptune, about the middle of the cycle. These planets made their commensurate perihelia, Uranus in 1714, and Neptune in 1718; consequently, the forces of these two planets, whatever influence they may exert on the earth in their perihelion approximations, were brought to bear in their greatest perfection during the cycle. Here is a philosophic reason. Jupiter and Saturn, Uranus and Neptune, were all in perihelion in this cycle—Jupiter five times, Saturn twice, and Uranus and Neptune once—all exerting their most powerful attraction, point-blank, upon our suffering world at one and the same time. This seems to indicate that, when the four largest planets in the solar system approach us instantaneously, the disturbance in the physical world is very great, and the epidemics are severely aggravated.

This is certainly interesting. Let us examine the reason offered for the greater severity and longer duration of this pestilential period a little more attentively. Uranus's period of revolution is eighty-four years; consequently, he must be forty years in making his perihelion circuit; and, as he made his perihelion passage in 1714, his increased attraction would extend forward to 1734, and backward to 1694. And Neptune's period being one hundred and sixty-four years, his perihelion circuit is eighty years in round numbers, and his increased power would extend from 1718 (his perihelion epoch) both forward and backward beyond the limits of the cycle under consideration. If this be not the cause of the greater length and severity of the period, and the planets have really nothing to do in the matter, the coincidences are very remarkable.

Example IV.—Let us go a step farther: the interest of the question looms up in the distance, as we pursue the inquiry back into the by-gone ages of the world. Such a period occurred (perhaps a still more severe one) in the sixth century, where Gibbon commences his flourishing description of the miseries of the human race, during the reign of Justinian I., followed in his "Decline and Fall of the Roman Empire," where it is asserted by the historian Evagrius that the plague raged for fifty years. And the historian Procopius calls it arrogance to pretend to assign the natural causes of this pestilence. Now, I have analyzed this period with care under the light afforded by the planets, and find that the malign cosmical disturbances opened with the commensurate perihelia of Jupiter and Saturn in 528, when a continued famine of eight years' duration succeeded. That the perihelion of Uranus followed ten years later, in 538, and that of Jupiter again in 540, and that in 542 the fifty-years' plague commenced. One might consider, indeed, a famine of eight years' duration sufficient without any planetary coöperation at all. But this is not all: I find that Saturn made his perihelion passage again in 557, and Neptune his in 566, and that Jupiter's returning periods were 540, 552, 564, 576, and 588, which last was his commensurate period again with Saturn's return. And I find that the periods of greatest mortality were mostly coincident with Jupiter's perihelion approximations. "The years remarkable for mortality are specified by historians, viz., 542, 543, 547, 558, 562 to 565, 582, 583, 587, and finally one of the most destructive periods of all was 590 and the few following years."[1] Now, four of these seven periods named occurred under three of Jupiter's approaches, to wit: 542–'43, 562 to 565, and 587 to 590, these last two being in one and the same period. But the *special facts* of the example are, that the cycle is precisely that of Jupiter and Saturn's commensurate cycle; that the perihelion opened under Jupiter's first approach in the cycle, and closed with the commensurate perihelion of Jupiter and Saturn, and that Uranus and Neptune were both in perihelion during the cycle. These are the astronomical facts that give force to this example. And this, by suggestion, calls up another example.

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[1] "History of Pestilence," vol. i., page 100.

EXAMPLE V.—Webster, commenting on Gibbon's flourishes, thinks there was no greater mortality in the latter half of the sixth than in that of the sixteenth century. I have analyzed this long period also, and find that all four of the large superior planets made their perihelia about the middle of the century—Uranus in 1546, Neptune in 1553, and Jupiter and Saturn made their commensurate perihelia in 1560–'61. So the astronomical causes of pestilence certainly bore as heavily, and as coincidently so, on the earth in the sixteenth as in the sixth century.

And here I may as well announce my belief that there is a sable period not far distant in the future, which I will digressively set forth as my sixth example, although it has not yet come to pass. I say a sable period—I may be too fast. There may not occur the same phenomena under the similar configuration of the planets in the future as have occurred in the past five examples given. I will simply state what the configuration of the four large superior planets will be in the proximate future, and leave room for the imagination of the reader to play upon the consequences that may follow:

EXAMPLE VI.—The next perihelion passage of Jupiter will occur in 1880; there will be a conjunction of Jupiter and Saturn in 1881, the commensurate perihelia of Uranus and Neptune will occur in 1882, and the perihelion passage of Saturn will occur in 1885, but not the commensurate perihelia of Jupiter and Saturn. This will be deferred until 1915–'16. But lively times in physic—lively times for doctors and undertakers also—may be looked for by those who believe in the certainties of astronomy, all the way from now to then, for Uranus will not complete his perihelion circuit until the going out of the nineteenth century, and Neptune not until 1923; so that malign influences may be looked for under every recurring perihelion approach of Jupiter during the cycle in which we are now sailing, viz., of 1880, 1892, 1904, and 1916.

The configuration of the four planets will be so remarkable in the coming period that I will notice it more in detail.

Jupiter will make his perihelion passage in September, 1880; Uranus his in August, 1882; Neptune his in December of the same year; and Saturn his in September, 1885—all within a period of less than *five* years, by a few days.

So close an approximation to the synchronous perihelia of the four planets has not occurred since the Christian era. The nearest approach to it was the period of 1708 to 1718 (Neptune's latest period of revolution), when they all made their perihelia within less than *ten* years of a synchronous adjustment. And this period, carried through to 1738 (the commensurate period of Jupiter and Saturn), was the most mortal period of the eighteenth century, as noticed under the third example, the period in which Lord Anson flourished as a circumnavigator, and his ships' crews nearly all perished of scurvy; long before the introduction of lemon-juice into the naval service.

Whether the nearer approach to a synchronous adjustment of the perihelia of these four heavy planets, that is to occur in the proximate future, will throw a heavier pall over the earth than the rather more remote adjustment of the past period spoken of, is a matter that time only can reveal. The planetary forces will be brought nearer to a focus; but the world is in an advanced condition. Agriculture, horticulture, commerce, the arts and sciences, medical skill and remedies, all the protecting comforts of life and health, are a thousand-fold advanced: the world ought to stand it better; and the most advanced and enlightened nations will ride out any great, forcible, and long-continued pestilential period better than they did a century and a half ago. But, although the middle period of the cycle we are in looks very threatening, there is more hope for the outcome in the commensurate period of Jupiter and Saturn, in 1915 and 1916, for the forces of Uranus and Neptune will have almost wholly passed by. Those whose expectation of life may be forty-five years at this time, may hope to live through the cycle. Young practitioners of twenty-five years of age now, who may ride out the pestilential gales of the whole cycle, and arrive at threescore and ten, will have some good experiences to offer on astronomical etiology, as to whether it be a fact or a fallacy. But let us consider some further examples that have already occurred:

Example VII.—Calculating backward from 1738, two periods of Saturn and five of Jupiter, we find they made their commensurate perihelia in 1679; and Jupiter's epochs in this cycle, calculated backward, were 1679, 1667, 1655, 1643, and 1631; all of which were well-marked pestilential periods, as any one may see by consulting Webster.

Uranus made his perihelion in 1630, just at the opening of this cycle, and no doubt gave auxiliary impetus to the malign cosmical phenomena of the early part of it.

Although I desire to be brief, I must touch lightly on Jupiter's periods in this cycle.

Period of 1631.—"Of the pestilence of this period there was hardly a suspension. Particular countries enjoyed short intervals of health; but Europe and America were severely annoyed by pestilential diseases between 1632 and 1637" (Webster).

Period of 1643.—"This severe winter (1642–'43) was followed by a very sickly summer on the Delaware River. Such was the mortality among the settlers from New Haven, who had not long been in that country, that it broke up their settlement. . . . In England, in 1643, a malignant fever was epidemic, and few escaped. In autumn it put on pestilential symptoms and petechiæ. . . . For the great mortality in England, through a series of years at this time, see the London bills" (Webster).

Period of 1655.—"In 1655 the plague was general in Europe. There died in Riga 9,000; in Amsterdam, 13,200; in Leyden, 13,000; in Naples, 240,000; survived 50,000. In the Neapolitan territories, 400,000; in Benevento, 9,000; survived 500. In Rome, 10,000. In Genoa, in 1656, 10,000; and, in 1657, 70,000, and 14,000 only survived" (Webster).

Period of 1667.—Under Jupiter's approach of 1667, occurred the last great plague in London, in 1665–'66, in which years the bills show a mortality of over 110,000. Sydenham flourished during this pestilential period, and his works may be consulted by those who wish to study this analysis more attentively.

Period of 1679.—This was the commensurate period of Jupiter and Saturn. The pestilential years continued after Jupiter's previous perihelion epoch, under the approach of

Saturn, in the same manner as they continued from the Irish-famine period to 1856.

"In 1670–'71, measles and small-pox were epidemic in London; in 1673, catarrhs and spotted fever; and in 1676, measles and small-pox. . . . In 1677, small-pox raged in Massachusetts with the mortality of the plague. In 1678, fevers and anginas were epidemic in Europe, and authors relate that four millions of people perished that year in Africa of plague" (Webster).

These historical items show very clearly the coincidence of aggravated pestilence with Jupiter's perihelion epochs through one of the commensurate cycles. They are, perhaps, more *point-blank* to time throughout this cycle than through any one other yet examined, and are the more striking on account of the bills of mortality cited; but really they are no truer to the fact than the coincidences of the cycle that culminated in the Irish famine, pushed through by Saturn to 1856. Look back on 1856, '45, '33, '21, and '09, all periods within the memory of those of threescore years and ten.

Example VIII.—And, lastly, calculating backward another cycle of fifty-nine years from 1679, we reach 1620. This was a commensurate period of Jupiter and Saturn, and the last example I propose to give. Jupiter's periods in this cycle were 1619, 1607, 1596, 1584, and 1572. This carries us back just three hundred years.

All of Jupiter's periods in this cycle were characterized by great disturbances in the physical world and severe epidemics; and the commensurate period was a very extended and mortal one.

"The Virginia colonies were nearly depopulated. Of 3,821 immigrants, only 1,800 survived. The North American Indians nearly all died. The Pilgrims found their bones bleaching above-ground in 1620" (Webster).

Uranus made his perihelion passage in 1630, the only additional astronomical cause that is patent for giving special force to this period beyond Jupiter and Saturn's influence. To be sure, his perihelion passage was ten years after this commensurate period; but the increasing force of his approach began in 1609, when he passed his descending node and commenced his perihelion career during Jupiter's previous perihelion circuit, and helped to roll on the ball till Saturn's approach commenced in about 1612. And Jupiter returned to the charge in 1616, when all three united their forces for a "long pull, a strong pull, and a pull altogether," their forces increasing till 1620, and that of Uranus till 1630. These compounded forces pushed the epidemics through *two* revolutions of Jupiter—in fact, from 1607 to 1631. For example:

"In 1611 the plague carried off 200,000 of the inhabitants of Constantinople. . . . This year (1614) was remarkable for the most universal small-pox, and most fatal ever known. It laid waste Alexandria, Crete, Turkey, Calabria, Italy, Venice, Dalmatia, France, Germany, Poland, England, and Persia. . . . In 1615 quartan-ague epidemics prevailed—not a family in Germany escaped. . . . In 1618 the plague appeared in Norway, in Denmark, and in Grand Cairo. This was the *beginning*" (italics mine) "of a very pestilential period" (Webster). And it continued till after Jupiter's next approach in 1631, coincident with Uranus.

This being the last example, I will touch briefly on Jupiter's periods of this cycle.

Period of 1619–'20.—Enough has been said of this period to satisfy the most incredulous that some great general cause was operating that aggravated the pestilence, and from the configuration of the planets shown, and their known power, what is more reasonable than to ascribe the aggravation to the combined and increasing attraction they exerted on the atmosphere and imponderables of the earth and its nervous inhabitants?

Period of 1607.—"In 1607 commenced an unusual concurrence of great agitations in the elements, and severe pestilence attended" (Webster).

Period of 1596.—"The years 1594, '5, and '6, were very rainy in England and Germany—crops failed, and in Hungary the famine was extreme. . . . In 1697 the plague was in Julier and Genoa, and there was a dearth in England. . . . In 1598, Persia, in Asia, was depopulated by famine, and Constantinople was almost stripped of its inhabitants by the plague" (Webster).

Period of 1584.—"In 1583 concussions of the earth occurred in England, and the plague appeared in London, Germany, and Holland. Diemerbroek mentions this as a pestilential year. . . . In 1685 appeared very malignant pleurisies. . . . The plague raged in Hungary, Austria, and Turkey" (Webster).

Period of 1572.—"This year the plague raged in Poland —at Basle, malignant fever—and an authority states that all maladies in France turned to epilepsy and palsy. . . . In 1574 the petechial fever, which had spread mortally over Europe, began to change into the usual form of plague. It made its appearance in London. . . . In 1575 the plague appeared in many parts of Europe, and raged with incredible mortality for three years (Webster).

It will be seen by these quotations that every period of Jupiter throughout the cycle brought an aggravation of pestilence; nor has the rule failed in a single instance during the three hundred years of our analysis, or rather, twenty-five periods of Jupiter, and ten of Saturn, or, better still, five commensurate periods of the two planets, viz., 1856, 1797, 1738, 1679, and 1620. So uniform and pronounced are the epidemics of the commensurate periods that the marvel is, the fact has escaped the attention of philosophers. The wonder is truly great that Webster, in his earnest search, should have overlooked the planets, and ascribed epidemics to the coming of comets. But astronomy is better understood in this day, and now is the time for a thorough investigation of this very important subject.

The attentive reader will have observed that I have only taken notice, emphatically, of every alternate perihelion approach of Saturn. It is because his first perihelion approach, in every cycle considered, occurs when Jupiter is in the aphelion circuit of his orbit, and the disturbance of the earth is inconsiderable in comparison to what occurs under their united synchronous approach. I have only sought to present the strong points of the argument, desiring to be as brief as possible. Saturn's single approaches, however, have a marked influence on the constitution of the seasons and years, and on the epidemics of the periods of his return; for example, take his single approach of 1826, when all of the other large superior planets were in their aphelion circuits, and, if they disturbed the earth at all, it must have been from *the want* of the stimulus of their attraction. And what was the character of the seasons and epidemics? The summers of 1825 and '26 were the two hottest summers that have ever occurred in America (Philadelphia observation), and the winter of 1826–'27 was one of the coldest ever known in America; and the mortality from cholera in the city of New York was nearly doubled in the years of 1826 and '27, and the total mortality of the city rose from 3,000 (round numbers), in 1823, to 5,000 (round numbers), in 1825, '26, and '27 respectively. Jupiter was point-blank in aphelion during those years; his perihelion epochs were 1821 and 1833.

By all the facts presented it is rendered very probable, to say the least, that Jupiter and Saturn's perihelion approaches do inaugurate epidemics; and that if Jupiter occupied the position of Mars, and Saturn that of the asteroids, their perihelion visits would tend to subvert the conditions of human existence on our globe. It is well their masses are so far from us, with such a multiplicity of asteroids between us to obtund and soften the severity of their unrelenting gripes. We need a nebular expanse indeed to shelter us. And if the asteroids are, as has been supposed, fragments of a magnificent world that once revolved in the regular Keplerian order of distance, but could not withstand the forces it had to encounter, and was shattered to atoms, which atoms now compose the asteroids, was it the united attraction of Jupiter and Saturn that destroyed it? We know that they have great attractive power. We know they are disturbing each other at every turn. We know that the planet Neptune was discovered by searching for the outside power that was all but drawing Uranus out of his orbit—one of the most brilliant achievements of astronomical science. And, although I do not subscribe to the hypothesis that the asteroids are the fragments of a rent world or shattered planet, or that such a catastrophe ever happened in the solar system, still if it did, I suggest the above as the probable cause, and the fact as a forcible illustration of the manner in which earthquakes and volcanic eruptions are caused, under the united attraction of the sun, moon, and planets.

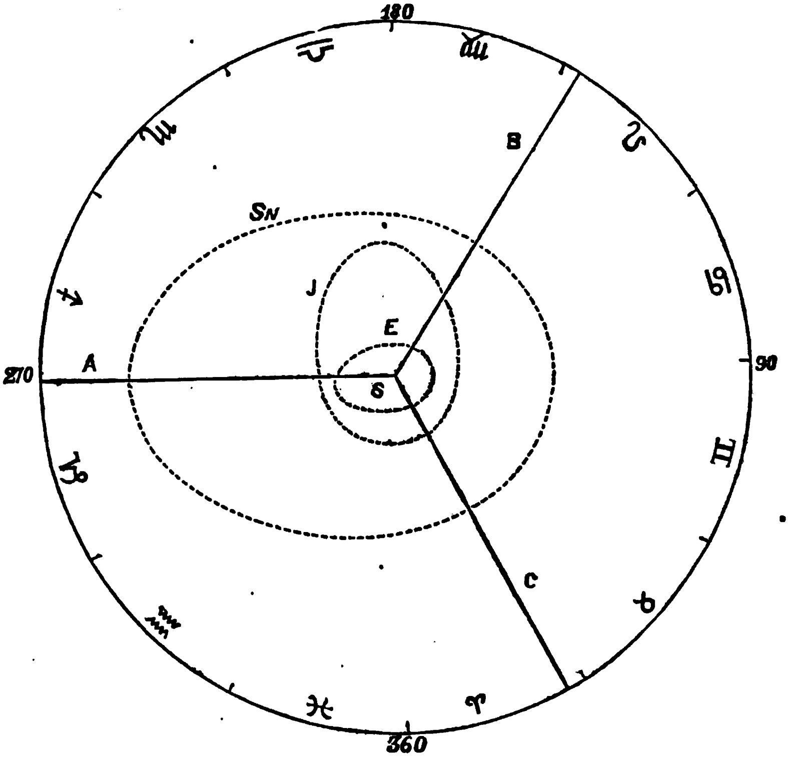
In studying the comparative influence of Jupiter and Saturn, it appears that Saturn's power for evil is less than that of Jupiter, inasmuch as his single perihelion visits do not cause the amount of disturbance that Jupiter's single visits cause. And very naturally, because his mass is less and he is much farther off. Still Saturn's power is great. The malign influence he exerts on the earth, when united with that of Jupiter, is made known by the increased aggravation of the commensurate periods over Jupiter's single periods, as in the records of the Irish famine and epidemics of 1843 to 1857, the continued pestilence of 1785 to 1798, or the yellow-fever epidemics of Dr. Rush's time; the scurvied epidemics of 1725 to 1745, the mortal period in which Commodore Anson flourished conspicuously, and which formed the basis of Lind's work on Scurvy; the great and continued pestilence of 1663 to 1679, covering the last great plague of London, and Sydenham's noble career and contributions to epidemiological literature; and the long-continued pestilence, again, of the period of the landing of the Pilgrims in 1620, say from 1607 to 1620, and even some years thereafter.

I will now illustrate by a diagram the configuration of Jupiter and Saturn during one of those long pestilential periods. We will take the latest of the series, the period of the Irish famine continued through to 1856. The epidemics and other cosmical disturbances of this most recent period are freshest in the memory of the reader. The configuration of the planets is the same, or varies but a little, during the three centuries through which I have calculated the examples. The diagram represents technically the epoch of 1738, when Jupiter and Saturn made their perihelia almost synchronously.

Let S in the diagram, p. 362, represent the sun, E the orbit of the earth, J the orbit of Jupiter, Sn the orbit of Saturn, and the lines A, B, C, three conjunctions of Jupiter and Saturn.

A conjunction of the planets took place near the line A in 1842 (some several degrees in advance of it), when the foul year began which inaugurated the famine and pestilence. Jupiter and Saturn were then passing over the higher apsis of the earth's orbit in conjunction, Jupiter's nearest approximation to the earth. According to the census of Ireland for

1851 (Mr. Yeates's Report), there were only eighteen months of average good weather in the British Isles from 1843 to 1849 inclusive.



Configuration of Jupiter and Saturn during the Irish Famine and until their Commensurate Perihelia in 1856.

Jupiter arrived at longitude 11° and made his perihelion passage in February, 1845, with great aggravation of pestilence throughout the world, potato-blights, etc., all of which is fresh in the memory of the profession.

During the very disastrous years of 1848 and 1849, Jupiter was passing over the lower apsis of the earth's orbit, and the minor axis of his orbit, his nearest to the earth in winter. The terribly severe winter of 1848–'49, and succeeding epidemics, must be well remembered by my readers.

Saturn was then in longitude about 360°, that of the minor axis of his orbit, and entering on his perihelion circuit. The climax of pestilence occurred in those years—cholera, typhus, and scurvy, ruling.

In 1851 Saturn passed the line C, and was in *opposition* with Jupiter; the foul years and epidemics continuing under that opposition and Saturn's approach, although Jupiter was in aphelion.

In 1853 and 1854 Jupiter was in opposition with the sun, passing over the higher apsis of the earth's orbit again, and the minor axis of his orbit, and entering on his perihelion circuit, and in the summers of those years was nearly 200,000,000 miles nearer the earth than in the winters when in conjunction with the sun. The hot and sickly summers and the excessively cold winters of those years must be well remembered by many of my readers, the yellow fever in New Orleans in 1853, and general pestilence of 1854 in the United States.

Both Jupiter and Saturn were now rapidly making their perihelion approximation. They made their commensurate perihelion in 1856, Saturn in March, and Jupiter in December. The yellow-fever ravages in Norfolk in 1855, and repetition of its terrible violence in New Orleans in 1856, must be well remembered by my readers, as well as the general pestilence of that period.

After this the foul years and general pestilence gradually rounded off as both planets retired, the sickly and weakly having generally fallen victims. No general pestilence afflicted the earth after this, until Jupiter's approach toward his perihelion again, in 1865, some three years anterior to his perihelion passage in 1868.

This is the configuration that these planets have made during each of the long pestilential periods which I have analyzed, and given as examples in this paper.

It is proper to say in this connection that the planet Neptune entered on his perihelion circuit in about 1842, and was probably accessory to the malign cosmical phenomena of the Irish-famine period. His disturbing power, to be sure, is small compared with either that of Jupiter or Saturn; still, when compounded with that of both, it becomes an item of account.

Uranus was in the aphelion circuit of his orbit. He entered on his perihelion circuit about 1860, and probably aggravated the late pestilential period, from which we cannot emerge until after Jupiter's return in 1880, because of the apapproach of Uranus and Neptune.

In addition to the proofs of planetary attraction already presented, I must call the attention of the reader, before closing, to the important facts (probably generally known) of the reaction of the planets on the sun, and also their effect on the magnetism and electricity of the earth.

The periods of the clustering of the spots on the sun have been observed to attain their maxima at intervals corresponding, *almost precisely*, with that of Jupiter's revolution; and also the period of the maxima of auroras and mean diurnal variations of the magnetic needle, inferred to be effects of the attractive force of the planets, Jupiter's mass greatly ruling.[1]

It does not appear, however, from Loomis's table of curved lines, that the maxima of these planetary disturbances always occur on the identical years of Jupiter's perihelia, nor always within the years of the range of his perihelion circuits. They frequently do, however, as seen in his table; and, when they do not, we are not warranted in concluding that the hypothesis has failed. The observations on which the table rests may not have been perfectly accurate. Nor can we say positively as yet, for the want of data, how much the *maxima* results may be hastened or retarded by the compounded action of the other planets with Jupiter, or, indeed, by the action of comets. The maximum of pestilence seldom occurs on the very year of Jupiter's perihelion epoch. The maximum of pestilence during the Irish famine came in 1849, when Jupiter was fast going into aphelion. And, moreover, the *conjunctions* and *oppositions* of Jupiter and Saturn must be taken into the account.

There occurs either a conjunction or an opposition of Jupiter and Saturn about every ten years, which may tend to the anticipation of maxima results. I am satisfied, from observation, that the conjunctions and oppositions of these heavy planets must be taken into account in any scientific calculation of the disturbances caused on the sun or on the earth by their attraction.

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[1] *See* Harper's Monthly for June, 1869, article Aurora Borealis, by Prof. Elias Loomis, of Yale College.

I have said a conjunction of these planets took place near the line A in 1842. A previous one had occurred in 1822, near line C, and one took place near line B in 1861. One will occur near line C in 1881, and another near line A in 1901, preparatory to the commensurate perihelia of the two planets in 1915 and 1916.

The oppositions also occur at intervals of about every twenty years, but on the intermediate decades, as in 1831, 1851, 1871, 1891, and 1911.

These conjunctions and oppositions may tend to give a more decadal character to the recurring periods of epidemics. The decadal character of epidemics has been noticed by medical writers of Great Britain, particularly by writers on continued fever.

I have not spoken of the disturbing influence upon the earth of either Mars or Venus. Mars is so small a planet, and his periods of revolution so frequent, that no marked influence can be set down to his charge; and Venus, although she disturbs the earth greatly, especially in her lower conjunctions, can only be considered as an aggravating cause of malign disturbances, meteorological phenomena, and epidemics, and not as originating a series of epidemical years.

Neither have I deemed it necessary to speak very particularly of the influence of the earth's perihelion approaches toward the sun; nor of the full lunar influence upon the earth, which is very great, and often, no doubt, aggravates the malign cosmical phenomena, particularly earthquakes and volcanic eruptions, when the moon makes her nearest approaches to us. An exhaustive paper, covering the astronomical influences for good and evil upon the earth, has not been my object, but rather to invite inquiry by presenting a few strong points and striking coincidences of planetary perihelia and pestilence. Should my researches stimulate others to investigate the subject, my object will be fulfilled.

Inasmuch as I am only an amateur in the science of astronomy, I deprecate the criticism of professed astronomers. I do not claim the most exact or *mathematical* accuracy for my astronomical calculations, having, for brevity of work, left out some of the decimals in the tables of the planetary elements. The years of the planetary perihelia being ascertained (which I believe to be the case in every instance), my purpose has been answered, without pushing the epoch to the hour and minute. If the principles set forth be true, the errors and inelegances of a tyro will receive the indulgence of all progressive minds.

ART. II.—*Recent Researches in Electro-Therapeutics.*[1] By GEORGE M. BEARD, M. D.

FIVE years ago, in a paper before the Medical Society of the County of New York, I had the pleasure of formally introducing electro-therapeutics to the profession of America. What was then sown in weakness has now been raised in power. The progress made in these five years is certainly greater than in the whole century preceding. The gospel of electro-therapeutics is now preached not only in all the great centres of science, but in all lands, from Russia to Japan, where men attempt the scientific treatment of disease, and in no other land has the advance been so rapid, original research so active and successful, and popularization so wide and so generous, as in America. The stone which the scientific builders rejected has become the head of the corner. Like a fire on the prairie, it has spread over the land, until what was a by-word and reproach, has become the rage and the fashion. It seems eminently fitting that the nation which first brought lightning from the sky, and which first made it the fleet messenger of civilization, should also be the foremost in applying this great force to the treatment of disease. Already an electro-therapeutical society has been started in New York; Boston and Philadelphia will very likely soon follow, and then one for the nation will be a necessary sequence. The exhibition of apparatus at this meeting of the American Medical Association would bear the palm over any country, and every month brings new improvements in our instruments and increasing skill in those who use them.

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[1] Portion of a paper read before the Medical and Obstetrical Section of the American Medical Association, Philadelphia, May 8, 1872.

In the brief time that can be allotted to a single paper on an occasion like the present, it will be useless to attempt any exhaustive *résumé* of the broad and growing subject of electro-therapeutics. I shall not even succeed in exhausting the subject of my paper, the "Recent Researches in Electro-Therapeutics." My hope is simply to present as tersely as possible a few points in electro-therapeutics to which my own personal experience has been specially directed during the last two or three years.

I must confine myself by necessity and by preference to what is new, or difficult, or doubtful and disputed, giving a wide berth to all propositions of general acceptance.

A New Method of using Electricity; Central Galvanization.—During the past two years and more, I have been gradually systematizing and perfecting a novel method of electrization, to which I have given the name *central galvanization.*

The results obtained by this method are so brilliant and important—in many cases over other methods are so decided—that I have thought best to here describe it in detail.

In *central galvanization the negative pole is applied to the epigastrium (the patient holding it by an insulated electrode), while the positive is applied over the head, around the sympathetic, and down the whole length of the spine, in such a way as to bring the brain, the pneumogastric, the spinal cord, and all the prominent plexuses of the sympathetic, indeed, the whole central nervous system, under the influence of the current.* I began to work up the method of *central galvanization* as early as 1869, but did not fully systematize the method, in all its details and in its completeness, until the following year. The method, as I now employ it, is a growth from small beginnings—a result of long and various experimenting.

Theoretical Considerations in favor of the Method.—Quite early in my electro-therapeutical experience, I saw that the single process of galvanizing the sympathetic in the neck, useful as it was in some cases, did not fully meet a large variety of the indications for which it was recommended and employed. I found the same difficulty with the special methods of galvanizing the brain, spinal cord, and pneumogastric.

Besides this, two other considerations induced me to seek for some more satisfactory method of galvanizing the central nervous system than had yet been devised.

1. The impossibility of *exclusively* localizing the current in the cervical sympathetic suggested the view that very likely the good results that in some instances followed the usual method of galvanizing the sympathetic in the neck were due as much or more to the effect of the current on the spinal cord or pneumogastric as on the cervical ganglia of the sympathetic. That the beneficial effects of galvanizing the neck in cases of nausea, dyspepsia, and gastralgia, were due in part if not entirely to the effect of the current on the pneumogastric, was more than probable.

Conversely, I found it impossible to tell how far my attempts to localize the current in the pneumogastric, by placing one pole at the pit of the stomach and the other by the inner border of the sterno-cleido-mastoid muscle, was successful; and whether the benefit derived took place through the pneumogastric, the sympathetic alone, or through both combined, seemed beyond the power of mortal skill to determine.

Similar difficulties were experienced in the attempt to differentiate the effects of the galvanizing the brain; how much the results of applications to the head were due to the direct or reflex action of the current on the brain itself, how much to its action on the cephalic ganglia of the sympathetic, and how much to its action on the roots of the pneumogastric and the after-part of the spinal cord, seemed in the present state of the sciences of anatomy and physiology absolutely impossible to determine.

In galvanizing the spine we were puzzled by the same complications. The cervical, thoracic, and abdominal ganglia of the sympathetic, with their enormous plexuses, are all liable to be affected by the current whenever it is applied up and down the spine; and how far the beneficial results of galvanization are due to the effect of the current on the cord itself, and how far to its effect on these ganglia and plexuses, only a special revelation can determine.

Still further, the subject is complicated by the consideration that electricity works powerfully by reflex action, and in galvanizing the brain, the cervical sympathetic, or the spine, reflex action must continually take place through the nerve-centres, and the therapeutical results produced by such treatment must be in part attributable to such reflex action.

2. A large proportion of the most frequent and distressing chronic diseases, as hysteria, hypochondria, neurasthenia, chorea, epilepsy, nervous dyspepsia, neuralgia, and many forms of insanity, are so obscure and subtle in their pathology that it is impossible to determine the precise seat of the disease in general or in any given case, and consequently we can never know just where the current should be localized. Even when the seat of the disease is, or is supposed to be, accurately known, if a special revelation should kindly inform us whether epilepsy, for example, takes its origin in the brain or in the sympathetic, and should point out to us just where the lesion occurred, we should still be in the dark in regard to the best method of localizing the current, for, without another and still more complex revelation we could not determine the extent to which all other parts of the nervous system had been affected by the local disease.

The force of this objection to the use of the accepted method of galvanizing the brain and cervical sympathetic is seen when we attempt to give the complete pathology of any of the diseases I have just mentioned, and, indeed, of almost any nervous disease that can be mentioned. Where is the precise seat of the disease in nervous dyspepsia? We know that the stomach is weak, and we prescribe galvanization of the pneumogastric; but what has the solar plexus and the spinal cord to say in the matter? Who can tell just how not only they, but the brain itself, may be the origin of nervous dyspepsia, or how much it shares in the pathological disturbance, and consequently how much it needs treatment? After eleven centuries of medical study, who can tell the precise and exclusive seat of the disease in epilepsy, hysteria, and neurasthenia? Is not the probability continually growing stronger with the advance of science, that in these and many other diseases the whole or a large part of the central nervous system shares as a cause, or result, or concomitant? Even in those diseases where the lesion is understood, is there not much more of the unknown than of the known? In locomotor ataxy, progressive muscular atrophy, spinal congestion and irritation, is the spine only at fault? Do the sympathetic and brain wholly escape the infection? "Evil communications corrupt good manners" in pathology as well as in morals, and the communications between the sympathetic, and cord, and brain, of the nerves that branch from all these are so varied, and intimate, and complex, that when the cord is known to be diseased we very naturally incline to consider the other parts of the nervous system, like "poor dog Tray," in bad company, and we become very justly suspicious of their character. In this suspicion we are justified by the accepted views of the functions of the sympathetic, and by the clinical signs and symptoms of these diseases.

In cerebral hæmorrhage we always know the general if not the precise root of the disease. The spinal cord, through disuse, becomes affected with secondary degenerations, and the organs of digestion also more or less sympathize.

Moved by these two considerations, and encouraged by some good results that followed the treatment of cases of gastralgia by placing one pole at the back of the neck and the other over the pit of the stomach, and at the inner border of the sterno-cleido-mastoid muscle, I extended the applications to the head and spine, and thus gradually systematized and perfected the method which I here call *central galvanization.*[1]

Details of the Method.—The negative pole is placed on the pit of the stomach, for the twofold reason that it is well borne there, and that a descending current seems to act better in most cases than the ascending. Whether the differential action of the ascending and descending currents is due to the *direction* of the current, or to the poles, I am unable to say. The positive pole is less sensitively felt than the negative, and is less irritating, and it is not unlikely that this fact may explain the more satisfactory results of the descending currents in central galvanization.

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[1] I first called the attention of the profession to central galvanization, in an article on "Electricity and the Sphygmograph," in the Medical Record, December 15, 1871.

It will be seen that the reasons here given for generally placing the negative pole at the pit of the stomach are the same that we have elsewhere given for placing the negative at the feet or at the coccyx in general faradization.[1]

In some systematic comparative observations that I made at Demilt Dispensary, the reverse method—placing the positive pole at the pit of the stomach—did not seem to be satisfactory, and similar experiments with the positive pole at the coccyx or at the feet in general faradization led to a like conclusion. The only way to determine a question of this kind is by comparing many applications on a variety of patients; in individual cases, no difference can be traced in the effects of the ascending or descending currents.

I do not always make the applications all over the head, but merely on the forehead, gently passing the electrode from one side to the other; then baptize the patient on the *cranial centre*, at the top of the head, and rest the pole there for about one minute, and sometimes longer. To the head I apply from two to six or eight cells—for patients vary in their susceptibility—and beginning with a weak current, and gradually increasing until a sour or *metallic taste* is perceived in the mouth. The *cranial centre*—the summit between the ears—I regard as the most important region of the head in all electrical applications, and especially in central galvanization. A current passing from that point to the epigastrium, traverses the centre of life—if life has any centre—and affects the sympathetic, and the roots of the facial nerves. The sensation produced by this application is different from that of any other application to the head, and is sometimes indefinable.

An application to this point for one or two minutes is usually about as much galvanization as the brain needs. In exceptional cases, where the hair is thin, or the head is bald, I make the applications all over the surface, back and front. In applications to the head, care should be taken to avoid sudden interruptions, or shocks that cause dizziness; the flashes of light before the eyes are of little account, but nothing is gained by producing them, and they are annoying to the patient.

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[1] Beard and Rockwell's "Medical and Surgical Electricity."

The electrode is then passed down the inner border of the sterno-cleido-mastoid muscle, from the auriculo-maxillary fossa to the clavicle, for the purpose of affecting the pneumogastric and sympathetic. I usually make the application on both sides, and from one to five minutes.

In galvanizing the spine, especial attention is given to the *cilio-spinal* centre, below the first and seventh cervical vertebræ, which is to the spine what the cervical centre is to the brain. The cervical sympathetic and pneumogastric, as well as the spinal cord, are affected by the current. The electrode should also be passed the entire length of the cord by *labile* applications up and down. The back is not usually sensitive, and strong currents, from ten to thirty cells, can be borne without any more discomfort than a burning or pricking sensation beneath both electrodes.

The back may be treated from three to six minutes, and the whole length of the *séance* of central galvanization ranges from five to fifteen minutes.

Preparation of the Patient.—All the preparation a *male* patient requires for central galvanization is to unbutton the collar, remove the coat and vest, and slip off the whole clothing, so that free access can be had to the spine.

A *female* patient may remove her corsets and slip up her under-clothing, or merely loosen the clothing at the neck and waist, so as to make room for an electrode to be passed down to the epigastrium, and for a spinal electrode to be passed up and down the back.

Electrodes.—For the negative electrode at the pit of the stomach any electrode with a broad surface, so as not to be too painful, and an insulated handle that the patient can hold, will answer.

For the positive pole, I prefer my *adjustable* electrodes, of different sizes. These can be passed under the clothing with great ease, and are wonderfully convenient for many of the purposes of electrization. They can also be provided with flannel covers, that may be washed as often as necessary.

Central Galvanization compared with Localized Electrization of the Nerve-Centres.—Before I had perfected the method of central galvanization, I had endeavored to fulfil the same indications for which it is required, by successfully localizing the galvanic current in the brain, the cervical sympathetic, the pneumogastric, and the spinal cord, by the usual methods of galvanizing these parts. The results, though sometimes all that could be expected, are not very far inferior to those obtained from central galvanization, and for these two reasons: 1. In their localized applications, both poles are brought to bear on the different parts of the nerve centres, and the irritating effect of the negative is very frequently injurious. 2. The successive localizations in the nerve-centres are very inconvenient for the patient, and very laborious for the operator, since they require constant change of the position of the patient and of both electrodes.

Central Galvanization compared with General Faradization.—General faradization is the method of all other with which central galvanization would be most naturally compared, since it is used for very many of the same general indications as well as for the same special affections.

The leading difference between them is, that *central galvanization chiefly affects the central nervous system*, while in general faradization *a large part of the muscular surface of the body is acted on.* All other conditions being the same, central galvanization is differentially and specially indicated in those nervous diseases by whatever symptoms expressed, or by whatever name indicated, where, in spite of the nervous exhaustion or perturbation, the *muscular strength and the general nutrition* are comparatively undisturbed. In many forms of aneuric disease, such for example as neurasthenia, hysteria, insanity, neuralgia, sick-headache, etc., the muscular development and capacity may not only be impaired, but *positively increased*, so that the patient can take very long walks and undergo a vast amount of physical toil without fatigue; such cases are most benefited by *central galvanization.*

On the other hand, when these or other diseases are accompanied or followed by loss of body weight, and by muscular flabbiness and feebleness, *general faradization* is indicated for the obvious reason that it is the most powerful method now known of developing the muscular system. In general faradization the central nervous system is, of course, affected, but to a less degree than in central galvanization. The practical difference between the two currents—faradic and galvanic—is mainly a difference of degree, due to the same difference that there is between bromide of potassium and hydrate of chloral, the faradic current being the bromide of potassium, and the galvanic the hydrate of chloral.

Bromide of potassium is a safer remedy than hydrate of chloral, fulfils a wider range of indications, but there are very many cases where it is powerless, and the hydrate of chloral acts as a specific; so the faradic current is safer than the galvanic, and therefore better adapted for general use, and, for those who use but one current, fulfils a larger requirement; and yet there are many cases when it fails and the more powerful galvanic is demanded.

It is this superiority of *degree* that makes the effects of central galvanization so much more positive and certain than faradization, however administered, in cases of nervous diseases of all kinds, and especially in gastralgia, angina pectoris, neurasthenia, and spinal irritation.

Central galvanization is indicated in very many of the same diseases as general faradization, but is differentially indicated in those conditions where the brain, spinal cord and sympathetic, pneumogastric and the large plexuses are involved. Thus in hysteria, hypochondria, insanity, gastralgia, angina pectoris, chorea, and spasmodic affections, nervous dyspepsia, where the system has not been greatly debilitated; in spinal and cerebral exhaustion, spinal irritation and congestion, and in certain diseases of the skin, central galvanization I have found, on the whole, more efficacious than general faradization. In some cases of hysteria, neurasthenia, anæmia, and in nervous dyspepsia when the weight of body has been greatly reduced, and in general debility of various kinds, I have found general faradization, on the whole, superior to central galvanization. Some of the very best results have been obtained by *alternating general faradization with central galvanization*. In some cases it happens that a change from one method to the other is of great advantage. Some patients, with strange caprice, act better under the one than under the other at one stage of their disease, whatever it may be, and at some other stage the conditions are reversed.

I have judged of these methods by the statements of the patients during the *séance* or directly after the *séance*, in the intervals and at the end of a course of treatment. I have judged by the appearance of the patients, by their changes in weight, size, and color, by the variations in the pulse, the temperature, the general circulation, the vascularity of the retina, by the relief of pain, the improvement in sleep and digestion, by the increased capacity for muscular and cerebral toil, and by the local effect on the tissue as manifested to the ear of the observer.[1]

In general, it may be said that *powerful stimulating tonic effects* are produced by both methods. The improvement in sleep and appetite, and in capacity for muscular and mental toil, and exhilaration, temporary and permanent, are observed either at the close or in connection with central galvanization, as after general faradization. *Increase of body-weight* is, I think, more marked after general faradization.

ART. III.—*The Electric Bath.* By ALEXANDER MURRAY, M. D.

A VESSEL made of good one-inch or inch-and-a-half pine-boards, free from knots, and of the following dimensions, will answer all the purpose of an electric bath-tub. It should be at least six feet in length, eighteen inches in depth, and twenty inches in width, and with a trap at the bottom of one end to empty the contents of the tub when required. A plain oblong square in shape is probably the best and cheapest arrangement. For a permanent fixture, a strong copper wire, (No. 14), or a brass rod, should run across each end inside of the tub, and through one of the sides, and about one inch and a half from the top. It should be so arranged on the side of the vessel that each wire or rod should have a finer one of pure copper, or insulated with either gutta-percha or rubber tubing attached, to form a connection with the poles of the battery. The brass rod or wire should be strong enough to sustain the weight of the metal element when lowered into the water, or when raised up and not in use. It is immaterial whether we use two carbons, two coppers, or zincs. But yet it is better to have one of zinc and one of copper or carbon, for the different poles. The zinc piece should be one-sixth of an inch in thickness, and eight inches by eight in width. The copper should be one-eighth of an inch thick, and about the size of the zinc plate—the carbon half an inch thick, and six by nine inches wide. Whatever plates are used, they should be arranged one at each end of the tub, and attached with a piece of copper wire or brass chain, and with hooks to suspend them to any depth required in the water, or by means of a brass chain of sufficient length, one end of which should be attached to the metal plate, and the other end made with a loop or ring to allow it to be suspended from a hook or button on the end of the tub, and a cord of copper wire attached to form the connection with the electro-magnetic machine or galvanic battery. It is always better not to allow the metal plates to touch the bottom of the bath-tub; about half an inch from the bottom will suffice.

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[1] Dr. Meredith Clymer informs me that, for the year past he has been in the habit of directing the galvanic current through the central nervous system by a method substantially the same as central galvanization. It is a fact of interest that, although he arrived at this method later than I, he yet devised it entirely independently, and used it for many of the same indications and with equally satisfactory and remarkable results.

The Bath.—Fill the bath-tub to one-third of its depth, or with about as much warm water as will cover the body when lying flat in the bath. The temperature of the water should range from 86° to 90° Fahr., according to the season of the year. The water should either be pure or impregnated with two or three handfuls of common table-salt, or it should be slightly acidulated with nitric or hydrochloric acid. From one to two ounces of the strong acid will be found amply sufficient for a bath. Whatever agent is used, it should be thoroughly mixed with the aid of a piece of wood, before the patient enters the bath. A small bench will be absolutely necessary for the purpose of supporting the patient's head while in the bath. This will be found a more convenient way than having one end of the tub bevelled to an inclined plane toward the centre of the vessel. The bench should be four and a half inches high in the front, and five and a half inches in the back, five wide and fourteen long; the top board should be perforated with half-inch holes two or three inches apart, and then covered with flannel, felt, or sponge. The best way to test the strength of the electric current before the patient enters the bath (as warm water and wet wood are good conductors of electricity), having first secured the connections between the tub and the electrical apparatus, is, to set the battery working at the supposed strength required, and then put the palms of the hands together and dip them in the water about the middle of the tub; then separate the hands slowly, keeping them and the fingers on the stretch well in the water toward each pole or end of the bath-tub. This simple process will enable us or the patient to judge whether the electric current is sufficiently strong or not, by the manner in which it acts upon our hands. It should at least produce a pleasant and agreeable sensation, without causing any cramps or pain in the wrist-joints. The patient, after he enters the bath, should lie on his back, and allow his feet to press against the suspended metal plate placed at the foot of the tub. The other element, supported by the copper wire, should be placed near the head, or it might have a long, insulated wire attachment to allow the plate (which should be previously covered with one or two folds of flannel) being applied to any part of the spine *below the shoulders;* or the patient might also use a large sponge-covered electrode attached to the positive pole of the electro-magnetic apparatus, and the current applied with either hand to any part or parts of the body desired.

From my experience and personal use of the electric bath during the last five years, I find it a pleasant, invigorating, and useful adjunct in the treatment of diseases. I can speak with a degree of confidence and without speculation as to the beneficial effects in all forms of rheumatism and allied diseases, hysteria, chorea, paralysis, amenorrhœa, diseases of the bladder, cholera-morbus, cholera infantum, and wasting diseases of children, etc., etc.

The length of time to remain in the bath should range from ten to twenty-five minutes. For children, five to ten minutes will be sufficient (children are so delighted with its pleasant, exhilarating sensations that it is difficult to get them to leave the bath). The best time to take the bath is between breakfast and dinner, or a short time before retiring to bed. It may be taken at any time when a person may not feel in his usual health, or during the prevalence of an epidemic, or any unusual electrical state of the atmosphere—but never when the stomach is full of food. There need be no apprehension of catching cold after taking the bath, by going out directly in the cold air. In using the galvanic current, and to avoid giddiness and other unpleasant sensations, we should place the positive pole at the feet, and the negative at the head.

If a physician wishes to apply electricity to his patient while in the bath, all that he need do is to reduce the quantity of the water in the bath-tub, so that the upper surface of the body is uncovered of water, then let him cover the body with a large towel well wet with the water of the bath, and commence the application of electricity at the head downward over any part or parts of the body desired, by means of a large sponge-covered electrode connected with the positive pole of the battery. The patient should at the same time keep one or both feet pressed firmly against the metal plate at the foot of the tub.

Clinical Records from Private and Hospital Practice.

I.—*A Case of Vesico-Vaginal Fistula, with Eversion of the Bladder, in which the Tongue-and-Groove Suture was used.* By W. W. Keen, M. D., Surgeon to St. Mary's Hospital, Philadelphia.

Mrs. B., aged twenty-seven, was confined June 1, 1871. She was over two days in labor, and the child was finally delivered by the forceps. Immediately after delivery she observed the urine passed away by the vagina, and she has ever since been suffering both from the discharge and eversion of the bladder.

March 13, 1872, she was admitted to St. Mary's Hospital. The first thing observed was her waddling gait. When placed upon the table it was at once explained by a red inflamed tumor as large as a large orange, projecting from the vulva. On a careful examination this was found to be the bladder, which had inverted itself through a large vesico-vaginal fistula, first into the vagina and then outside of the vulva. The mucous membrane of the bladder on the surface of this tumor resembled the eyelids in "granular lids," and was more or less coated with shreds of mucus.

After a week's rest strictly in the recumbent posture (in which posture the bladder retracted into the vagina), and the continued use of the sedative injections, the parts were carefully examined, and the following conditions found:

The forceps had evidently not been the cause of the fistula, but the continued pressure of the child's head. This had produced a circular slough all around the vagina. Where this slough corresponded with the bladder it had destroyed the entire vesico-vaginal septum; elsewhere it had simply destroyed the neighboring soft parts (the rectum being fortunately not penetrated), and in healing had produced a circular cicatricial band which was interrupted only at the fistula, or, rather, here it followed the borders of the orifice. The cicatrix was so contracted that the small end of a Sims speculum could not be introduced beyond it. The fistulous opening was very large, as may be inferred from the protrusion of the bladder. It involved, in fact, all the *bas-fond* of the bladder corresponding to the vagina, and was about one inch in the axis of the vagina, and one and three-quarters inch from side to side. The upper margin of the opening on the left side almost involved the anterior lip of the uterus, only say an eighth of an inch of the vaginal wall being left; on the right side there was about one-fourth to three-eighths of an inch.

On March 20th I performed the usual operation, paring the surface at the margin of the opening freely and introducing seven wire sutures fastened with perforated shot, and introduced a sigmoid catheter. To obtain access to the parts, I nicked the cicatricial band in four places and distended it. The constant protrusion of the bladder into the vagina troubled me very much, but was overcome as well as could be by a long, stout, whalebone probang with a sponge on its end introduced through the fistula.

Three days afterward the catheter came out, and the resident was not informed of it for a long time. When reintroduced he drew off about thirteen ounces of urine which had accumulated in the bladder. The result of her carelessness was soon shown in the escape of urine by the vagina; and when I removed the sutures, on the tenth day, the orifice was quite as large as ever, if not larger. The sutures were also, I think, too few.

After rest, good diet, etc., had brought both part and system to a satisfactory condition, on May 10th I operated on the cicatricial band, paring it away, as I had found it to interfere with the free access I desired. I then introduced a large, round metal speculum, whose borders I perforated, and retained it in place by a double T-bandage. The resident had great difficulty in retaining the speculum in place, as the band was so high up that on even slight movement the speculum slipped below it. It healed over, however, very well, and the calibre of the vagina was greatly enlarged.

May 21st.—I operated on the fistula a second time, and after a method suggested by Dr. John H. Packard, who kindly assisted me, with my colleagues. On the left side there was but very little space between the fistula and the os uteri, so that a sufficient raw surface could not be obtained without encroaching on the anterior uterine lip. Dr. Packard suggested that, after freely paring the vaginal surface of the lower margin, the upper edge should be split transversely, thus produ-

FIG.1

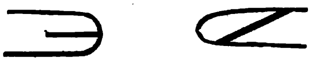
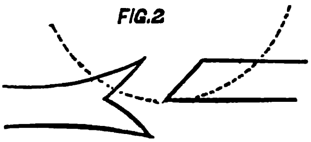


FIG.2



cing a wound which should be united, as shown in Fig. 2, by inserting the lower margin between the lips of the upper margin as a "tongue and groove," the dotted line representing the suture; not only would the needful two raw surfaces be in apposition, but the lower lip in the drawing (as the patient lay on her face) being on the vesical surface would prevent the ready access of the urine to the point where union was desired, and also none of the tissue at the upper margin would be sacrificed if, in case of failure, another operation was desired. I introduced at this time fourteen sutures. I also was specially careful at the two corners of the orifice to see that exact union was obtained. The catheter was again introduced.

As after the first operation, no reaction followed. She could not have done better. The vagina was syringed twice daily with warm water, and afterward with flaxseed-water. From the eighth day some urine escaped from the vagina. I did not remove the sutures till the seventeenth day (in consequence of unavoidable absence), when I found that all the edges had adhered, and the fistula was closed, except at the right corner, where an orifice as large as a pen-handle remained.

July 2d.—The parts being again in good condition, I pared the edges of the remaining fistulous opening as usual, and introduced five sutures. On the 11th I removed them all, and found it entirely healed. The cicatricial band is reëstablished partly on the right, on the left scarcely at all.

July 17th.—I sent her home. The band she will probably have removed in the fall. On removing the shot after the second operation, May 21st, one was cut off entirely from its loop of wire, and the wire, in spite of diligent search, could not be found. I found it to-day, on digital examination of the vagina, and removed it without any difficulty. No inflammation or ulceration existed around it, though it had remained in the tissue for two months.

II.—*A Case of Caries of the Vertebræ and Degeneration of the Spinal Cord.* By Dr. C. Frommann. (Virchow's Archiv.) Translated by S. G. Weber, M.D.

H. Geist, mason, aged fifty-eight years, had suffered for thirteen years from frequent attacks of pain in the loins, and at length became unable to walk. Pains and abnormal sensations were felt in the legs. On March 22, 1864, there was no pain in the vertebræ on pressure, motion was gone, and sensation diminished or lost in the legs. In the feet and legs, as high as the knees, there was a sensation as if they were placed on ice. Reflex motions were excited on irritating the foot, leg, and thigh, with a sponge wet with warm or hot water. During the next month he became worse, and on May 15th died.

The brain was healthy. The vertebral column was curved in the dorsal region, the greatest prominence being at the eighth dorsal vertebra. The eighth dorsal vertebra was entirely destroyed, and the sixth and seventh partially destroyed by caries. In this region the dura mater was thickened, and over the eighth vertebra it projected inward. The spinal cord was here somewhat compressed, its consistence not diminished, rather increased; below the compressed portion to the lumbar enlargement the gray substance had a brownish-rust color, sometimes the anterior, and sometimes the posterior cornua being most affected; above the compressed point the posterior columns were secondarily affected, below the other columns. The spinal ganglia, between the diseased vertebræ and the ganglia of the sympathetic near them, were embedded in thick fibrous tissue, swollen, and, on section, of a bright-red or yellowish color.

The microscope showed extensive changes which, originating in the connective tissue and vessels, had led to destruction of a part of the nerve-fibres. The fibres of the neuroglia were swollen to double, treble, or quadruple their usual size, but there was only a slight increase of the nuclei; only a few amyloid bodies. These fibres were found more numerous in certain regions than elsewhere, and they rendered the space for the nerve-fibres too narrow, so that the axis-cylinders lay quite close to the septa. Where all or nearly all of the septa between the nerve-fibres were affected, there was appearance as of a solid patch of connective tissue pierced only by the axis-cylinders. On longitudinal section also the fibres of the neuroglia were seen swollen, and there was disappearance of the medullary layer of the nerve-fibres. Where the swelling of the fibres of the neuroglia was greatest, so that they were treble or quadruple their ordinary size, many of them showed clear, shining, sharply-defined granules, solitary or in rows or groups, and also very fine short fibrillæ, which often were connected with the granules or enclosed them, not always lying parallel to the axis of the fibres, sometimes projecting slightly beyond. Old swollen fibres of the neuroglia, hwen covered with these fibrillæ, looked like the old, unchanged neuroglia net-work.

The nerve-fibres within the diseased portions, besides change of volume, were more or less degenerated; some fibres were enlarged, measuring as much as 0.036 mm., the enlargement affecting the medullary substance chiefly, occasionally the axis-cylinder. In some of the fibres the medullary sheath had degenerated into a finely-granular mass. In longitudinal section the axis-cylinders were seen to vary in thickness. Other axis-cylinders had lost their sharpness of outline, and had a dull, finely-granular appearance. Here and there nerve-fibres had entirely disappeared, medullary sheath and axis-cylinders not to be seen, their place being occupied by a finely-granular substance, or by round or oval cell-like bodies.

The nerve-cells of the anterior and posterior cornua were unchanged.

The nerve-roots seemed unchanged, but with the microscope the nerve-fibres were seen to have undergone a change similar to that which the nerve-fibres of the white substance of the cord had passed through.

The walls of the vessels were thickened, not only in the parts of the cord most diseased, but also where no disease could be discovered, also in the nerve-roots. This thickening was not due merely to a thickening of the adventitia, but was at the expense of the surrounding tissue.

The processes of connective tissue which the pia mater sent into the interior of the cord were bordered by degenerated tissue until these processes gradually disappeared, when the degeneration was more widely spread. The pia mater itself showed no very great amount of change; occasionally there was an increase of nuclei in its outer layers.

The stroma of the spinal ganglia was here and there filled with nuclei, the ganglion-cells at these places had a much darker and muddier appearance, their nuclei sometimes being indistinct; others, besides strong pigmentation, contained large and small fat-globules. The ganglia of the sympathetic showed a similar change.

Of special interest are the round or oval cells with one or two nuclei which were found in the spaces from which the nerve-fibres had disappeared. The thickness of the walls of the vessels would be against the supposition that they were white blood-corpuscles. The possibility that, by means of these cells, the nerve-fibres might be regenerated, must not be rejected.

III.—*A Case of Necrosis of One-third of the Seventh Rib.*

By James S. Bailey, M. D., Albany, N. Y.

Several years ago, while living upon the Brazos, in Texas, a well-grown mulatto boy, fourteen years of age, was brought to my office for treatment. An examination revealed a fistulous opening through the sternal third of the seventh rib upon the right side, which seemingly communicated with the lung. When a probe was introduced, a severe paroxysm of coughing was induced, with expectoration of matter similar to that escaping through the opening externally. My diagnosis was necrosis of this portion of the rib, and that the lung had also become perforated by ulcerative suppuration, and that it had become united to the adjacent portion of the thorax. Complaint was made of a tearing sensation in this vicinity, upon taking a full inspiration.

It was a month before I saw his master, and in his absence I determined to give him a generous diet, and accordingly fed him from my own table.

During this interval he gained rapidly in flesh and strength, but still his cough increased, as did also the discharge of pus externally.

Upon my next interview with his master, I stated plainly my opinion, and recommended, as the only means of saving his life, the extirpation of the necrosed portion of the rib. The risk of the operation was fully explained; also the benefit resulting, should it succeed.

The master gave his assent, should the boy be willing, which he cheerfully was, and, after a few days' preparation, I proceeded to operate, assisted by Dr. N. N. Allen, of Hempstead. We anæsthetized him by the use of chloroform, and I made a free incision from the sternum fully one-third of the length of the rib, dissecting the integuments from the bone, which was found to be spongy, and, by means of a gouge, I succeeded in removing the necrosed portion of the rib, after which the cavity was filled with lint-cotton, and the wound brought together with adhesive straps.

He came from under the influence of chloroform nicely, and, after the excitement incident to its inhalation passed off, he was quite comfortable.

The cough and expectoration ceased in the course of a few days, and the wound healed without an untoward symptom.

Two years elapsed before I again saw the patient; he had nearly grown to manhood, and was seemingly in the enjoyment of vigorous health, and during this period had not experienced the slightest inconvenience from this source. Seeley was an intelligent boy, and from him I obtained the following history of his case, which was corroborated by his master:

For a year previous he had been living upon a cotton-plantation near the coast; was taken with pain in this region. A physician was consulted, who insisted that it resulted from a torpid liver. He was mercurialized, blistered, and kept upon an abstemious diet; was dosed every third night with a blue-pill for several months. There was no improvement. Another physician was consulted, who confirmed the former physician's diagnosis, and approved of the course pursued.

In about six months from his indisposition the parts became tumefied and sore, and finally broke externally and internally as he claimed, which was followed by loss of appetite and decline of flesh and health. He remained in this condition until his consultation with myself. The result of surgical performance was truly gratifying to the patient, master, and physician, and, had it never been resorted to, he undoubtedly would have filled a premature grave—"gone where the good niggers go."

IV.—*Paralysis of the Sympathetic and Ulnar Nerves from Gunshot-Wound.* By Dr. Seeligmüller. [Berlin. Klin. Wochenschrift, 1872, 4.]

Lieutenant T., aged twenty-five years, was shot in the left shoulder, August 30, 1870, at the battle of Beaumont, as he stood on a hill; the ball, coming from below, entered the clavicular portion of the left sterno-cleido-mastoid about three centimetres above the edge of the clavicle, and came out near the spinous process of the fourth dorsal on the left. The left arm was paralyzed, and there was pain in the course of the ulnar nerve, especially from the elbow to the hand. For eight days he spit blood.

The author saw him nine months after the reception of the wound; the left lids are less open and the pupil is smaller than the right; by rough estimate the left pupil is in the proportion 2 : 3 to the right; but, if both are shaded, the right is much more dilated, and is twice as large as the left. The left pupil reacts to light, but more sluggishly and less than the right. When agitated, the left eyelids are more nearly closed. There was no myopia on the left. The injection of the conjunctiva and the redness of the cheeks are generally equal on the two sides, but when excited the left is redder than the right. During the examination of the eyes the left alone watered. The ordinary sensations were normal on both sides. The left cheek was much emaciated. The right temporal artery is more tortuous and pulsates more strongly than the left. The temperature of the left ear was taken once, and was one-tenth degree C. higher than the right. The left superior cervical ganglion was not tender on pressure. The paralysis of the arm had become less, and was limited to the region supplied by the ulnar nerve.

V.—*Herpes Zoster.* By O. Wyss. [Arch. d. Heilk., 1871—Centralblatt f. d. Med. Wissensch., 1872, 7.]

The patient was a man sixty-eight years old; he had, 16th of September, headache and fever; 19th, an eruption of herpes labialis; then later, around the right eye, forehead, nose, and cheek; on the 22d, the right eye, and, on the 23d, spreading to the left; and on the 26th, he died.

The eruption occupied that portion of the face supplied by the first branch of the right trigeminus. The first branch of the right trigeminal was wider and thicker than the left, of a redder color and softer consistence; the single nerve-bundles were separated by a red, soft tissue, containing many vessels. The nerve showed this change from its entrance into the orbit to its finest divisions; the nerves not belonging to the trigeminus in the right orbit were normal. The first root of the trigeminus from the orbit to the Gasserian ganglion was surrounded by extravasation of blood; behind the Gasserian ganglion the nerve was normal. The ganglion itself was larger and more moist than the left; on its inner side was a red appendage about one centimetre wide, seemingly extravasated blood; the substance of the ganglion itself was bright red. The parts where the second and third branches were given off seemed to be less changed; at the origin of the first root the ganglion-cells were separated from each other by infiltrated pus-corpuscles, but there was no abscess.

VI.—*Case of Abdominal Extra-Uterine Fœtation.* By S. T. Satterthwait, M. D., Belmont, Ohio.

On the 27th of September, 1870, I was called to see Mrs. B., aged thirty-five, wife of a day-laborer. She had given birth to three children, two living. The third and last one was still-born. On inquiry I elicited the following facts:

About thirteen months from the above date she was delivered of an inanimate child, after a painful instrumental labor of eight or ten hours' duration. On the fifth day after delivery she was taken quite ill with what her physician termed childbed fever.

This she partially recovered from after a dangerous illness of five or six weeks. While recovering, and as the tenderness gradually subsided in the abdomen, she recognized the presence of a moderately large "hard lump," low down in the hypogastric region. About the middle of the fifth month after confinement, she was attacked with a severe diarrhœa—the evacuations being very large, extremely fetid, and of a mixed quality. The quantity passed gradually increased, until it amounted to one gallon per twenty-four hours. The loss of weight and strength was very rapid. The tumor began diminishing in size immediately after the diarrhœa set in. I found the woman in the following condition:

Great emaciation, hectic fever, prostration, small, frequent, feeble pulse, anxious, haggard expression of countenance, bowels acting every half-hour, attended with considerable tenesmus. Evacuations composed of blood, serum, some feculent matter, and a dark, matted substance which proved, on examination, to consist mainly of hair. The odor of the discharges was extremely offensive. On making a vaginal examination, an aperture, one-fourth of an inch in diameter, was discovered in the superior portion of the posterior wall of the vagina, through which oozed a dark, thick, offensive fluid. The extremities of several bones could be seen crowded against the inner side of the margin of the opening. A probe was passed through this opening into what felt like a sack of bones. On making a rectinal examination, a large opening, perhaps an inch in diameter, was discovered in the anterior wall of the rectum, about two and a half inches from the anus. This opening communicated directly with the above-mentioned sack of bones. During examination, a bone was removed from the rectum, between the anus and opening in the bowel, which proved to be the tibia of right leg of a fœtus. Ordered concentrated nourishment, alcoholic stimulants, and quinine in large doses.

On the 28th found the patient some better. Examined the accumulated anal discharges of the twenty-four hours, in which I found hair and two ribs. On making rectinal examination, found several bones impacted in the aperture between the sac and bowel. By the use of a blunt-pointed bistoury, I succeeded in enlarging this by making an incision from the lower margin of the opening downward in the direction of the long diameter of the gut. Ordered continuation of same treatment.

On the 29th found the patient much better in spirits and in strength. While at stools through the night, had passed a large quantity of thick, fetid matter, and most of the long bones of a fœtus. From the 29th of September until the 3d of October, there was no material change in the patient, with the exception of improvement in strength. While examining her on the 3d of October, she was seized with severe excruciating pain over the lower part of the abdomen through to the perinæum. A rectinal examination demonstrated the cause of the pain. One of the bones of the cranium, the occipital, had partly escaped into the rectum through the opening, and its serrated edges were in contact with the rectinal walls. A pair of bone-forceps were used, and the bone broken into several fragments, which were removed without any difficulty. All of the skull-bones were treated in like manner, as they successively presented at the opening. After a removal of the cranial bones, the patient began improving with great rapidity. The discharges from the bowels became altered in quality and diminished in quantity. They lost their peculiar odor, consisting of serum and feculent matter.

On the 10th of November found the patient enjoying what she styled good health. She was quite fleshy, weighing one hundred and eighteen pounds; appetite very good. All that time she was doing all the house-work, besides making split baskets. On making vaginal examination found the aperture in the posterior wall almost entirely closed up. A small opening remained, which admitted the point of a fine probe, and out of which oozed a few drops of healthy pus. Os and cervix uteri looked normal. On rectinal examination, found the opening in the anterior wall of rectum diminished in size; the opening still being large enough to admit the extremity of the index-finger. Her bowels were acting about four times in twenty-four hours, the discharges consisting of serum and natural-looking fæces.

On December 5, 1871, found the patient enjoying excellent health. A small, white cicatrix was all that remained of the opening in the posterior wall of the vagina. Still a small opening in the anterior wall of the rectum. Bowels acting regularly. Every third or fourth day two or three ounces of serum is discharged with the fæces.

March 10, 1872.—Patient still enjoying good health. Bowels acting regularly, with occasional serous discharges.

Patient states that she is again with child; became pregnant the latter part of February, 1872.

VII.—*Some Accidental Results of Cerebral Lesion.* By Dr. L. Fleischmann. [Jahrbuch f. Kinderheilkunde, 3, 1871.]

The author records the details of several cases, of which the following synopsis may be given:

1. A boy, twelve and a half years old, when three years of age, was frightened by a dog, became unconscious; subsequently for three weeks he had, during the night, an attack; lay in bed with frequent trembling and dyspnœa, then seemed to recover. Six months later had an attack of eczema on the left side of the face and head. He was easily frightened. About twelve years of age the left foot dragged, both feet being previously weak. The greater amount of disease was always on the left side; the left eye suffered from ophthalmia, the left side of the face eczema. Cerebral symptoms became more marked; pulmonary disturbance set in, and he died.

The left lung was free, the posterior and lateral portions showed numerous hæmorrhagic points, from a pin's-head to a lentil in size, under the pleura of a clear red. Nearly the whole pons was degenerated into yellow, cheesy tubercle; the right side seemed to have been the original seat of the disease.

2. A boy, eight years old, had symptoms indicating disease of the left cerebral peduncle. At the autopsy was found

a small tubercular mass at the base of the brain; in its neighborhood were small punctiform hæmorrhages into the gray substance and also into the white. On the surface of the kidneys there were also small hæmorrhagic spots.

3. A rachitic girl, eight years old, was found to have an encysted tubercular mass, the size of a pea, in the right hemisphere on a level with the corpus callosum, and a similar one in the anterior lobe of the left hemisphere. Over the visceral surface of the pericardium and the pleura were numerous bright-red hæmorrhagic spots the size of a pin's-head.

4. A boy, two years old, had a tubercular mass in the left optic thalmus; there were also numerous emphysematous spots in both lungs.

These cases were thought to be of interest in connection with Brown-Séquard's experiments, in which he produced emphysema and hæmorrhages in the lungs by wounding the pons or the crura cerebri.

Correspondence.

THE INTERNATIONAL OPHTHALMIC CONGRESS.

London, *August* 5, 1872.

The International Ophthalmic Congress, which has just closed its three days of work and festivities, may be fairly called a success. There were about a hundred delegates in attendance from England, Germany, Holland, France, Spain, Denmark, Ireland, Scotland, the United States, and Brazil. The great Donders, the man of all others, next to Helmholtz, who has done the most to advance the science of Ophthalmology, was present, was elected presiding officer, and contributed papers. I shall not attempt to give you any detailed account of the proceedings—that will be found in the *London Times and Gazette*, and in the *Lancet*, as well as in the *Boston Medical and Surgical Journal;* but I will venture upon a sketch of some of the prominent features of the sessions, as well as of some of the leading members of the Congress.

Mr. Critchett, who called the Society to order, is a bluff, hearty-looking Briton, with the typical side-whiskers of a former period. He and Mr. Bowman are probably the leading practitioners as oculists in London, although many of the younger men are quite as well known to your readers as writers. Mr. Critchett speaks excellent French, and is most pleasing in his manners. Mr. Bowman (of Todd and Bowman's "Physiology") is a quiet, gentle-looking man, whom one would be very apt in our country to mistake for a well-to-do, successful, and retired merchant.

Both Mr. Critchett and Mr. Bowman have evidently passed the shady side of fifty, but are both still in their full vigor. Prof. Donders is quite above the average size; he has jet-black hair and full whiskers. He looks like a stalwart American. Indeed, I observed in Holland that the type of figure and face was very like that of our own New York, which can hardly be said of any other part of the Continent. Soelberg Wells was the English secretary, while Zehender represented Germany in the same office. War~~c~~omont, of l Brussels, a brisk, white-haired Belgian of some fifty years, editor of the *Annales d'Oculistique*, and our own Williams, of Boston, were the vice-presidents. I was surprised to find Soelberg Wells, whom I had never before seen, to be a much older-looking man than his contemporaries in New York, although I think that his gray hair is not to be trusted as a sign of his age. He is quite large, is not very fluent or agreeable as a public speaker, but very pleasant in conversation. Zehender, the editor of the *Monatsschrift für Ophthalmologie*, is an extremely modest-looking, dark-featured German. In making an address to the Congress he scarcely took his eyes from the paper, and the mere mention of his name would cause a blush on his face. The tall form and graceful speech of H. W. Williams need hardly be described.

The Congress met in the Royal College of Physicians, corner of Pall Mall and Trafalgar Square, in the Library, and surrounded by the portraits of such eminent medical men as Harvey. The building is a stately one, and has a fine situation in about the centre of London. New York certainly needs such a place for the meetings of her various medical societies.

The first evening was spent in registration, when any respectable medical man, on paying a fee of twelve shillings and sixpence, who desired it, was enrolled. At the same time we were politely informed that there would be a banquet on Saturday evening at the Sydenham Crystal Palace, to which we would be admitted on payment of one guinea. I may say here that, on reaching Sydenham, we were obliged to pay two and sixpence each for admission to the dining-room, through the palace. On the way we got our money's worth, however, in the beautiful grounds, and in looking at the wonderful aquaria, where we saw submarine life on a scale probably nowhere else to be seen. The walking lobsters and crawfish elicited enthusiastic applause.

The work of the Congress was done in three days, in sessions twice a day, of about three hours each. Besides this, the various ophthalmic hospitals were thrown open, and very many operations performed with the skill for which English surgeons are so justly celebrated. Messrs. Bowman, Streatfield, Hutchinson, Critchett, Cowper, and Lawson, were among the surgeons at the old Moorfields Royal London Ophthalmic Hospital [a hospital which has some twenty thousand patients a year], who operated, while Mr. Bader was at Guy's; Mr. Power and Mr. R. B. Carter at Westminster; Mr. Liebreich at St. Thomas's, etc. Liebreich took no part in the Congress, although he operated every day. He did not even venture to present his *popular* hypothesis, first contributed to *Macmillan's Magazine*, in which he attributed Turner's peculiarities as a painter to his astigmatism of the artist. It can be easily shown that this hypothesis is not tenable, but it has undoubtedly caused Liebreich to be talked about in the clubs, and thus well advertised as an oculist. Liebreich is a slight, nervous, black-haired man, hardly the equal of the London men as an operative surgeon. The prevailing language of the sessions was English, and yet Donders presided in French, and very many of the papers, and all the main propositions in them, were read in that language. The debates were in French, English, and German, so that the sessions were quite polyglot in character. Eleven American ophthalmologists were present, Williams and Jeffries, of Boston; Agnew, Noyes, and

Roosa, of New York; Rider, of Rochester; Norris, Dyer, and Thompson, of Philadelphia; Williams, of Cincinnati; and Green, of St. Louis. It was decided by a large majority, after an animated debate, to hold the next Congress in 1876, in New York; a very eloquent Spaniard pleaded hard for Madrid, but New York was victorious, after invitations from the Williamses and Noyes, backed up by the entire United States delegation.

The dinner was a grand success. The toasts were brief and good, and the Congress broke up at about half-past ten P. M., with great hopes for a large attendance in New York.

Argyll Robertson, of Glasgow, was one of the finest-looking men of the Congress, and he read an excellent paper. I should say he was about forty-five, tall, with dark hair, and a very pleasing voice, with a pleasant Scotch accent. Wilson, of Dublin, author of a work on the ophthalmoscope, and Sir William Wilde's colleague, was also present.

Javal, a very German-looking man, a native of Alsace, was the only real Frenchman present. Wecker and Meyer were also delegates, but they are Germans, although living in Paris; while Galezowski is a Pole. The English provincial surgeons were represented by Solomon, of Birmingham, and others. Quadaligno, of Turin, appeared for Italy.

The cordial hospitality of the English surgeons, as evinced by their invitations to their homes, as well as by their frank and agreeable manners, will always be warmly remembered by the delegates.

D. B. St. John Roosa.

PROF. J. J. CHISOLM'S CASE OF SUPPOSED ANEURISM.

Editors of the New York Medical Journal.

In your issue for August, you publish, page 161, the "Report of a Case treated in the University of Maryland Hospital, Baltimore, a Supposed Aneurism in the Popliteal Space, without any Characteristic Symptoms by which to establish a Diagnosis. Service of Prof. J. J. Chisolm."

To this report is appended a note containing several gross inaccuracies, and reflecting not only upon me, but also seriously upon the surgical skill of the visiting surgeons to the University of Maryland Hospital.

"His term of surgical service having expired," reads the note, "the case fell into the hands of his successor, who, not having seen it before, considered the fistulous opening and polished skin evidences of malignancy, and amputated the limb."

In regard to the examination of the tumor after amputation, the note reads:

"The centre of the solid contents had undergone physical changes from two months' exposure, with surface liquefaction, and resembled the ragged appearance seen in cutting into a cancer, the centre of which had softened with open ulcer. But, with this appearance, analogy ceased, for," etc., etc.

"Dr. Chisolm procured a portion of this solid contents and submitted it to a skilled microscopist, who reported hematin in quantity, also narrow spindle-cells such as are seen in inflammatory new formations. After a most careful search, no cancer-elements were found."

The natural deduction to be drawn from the above is, that "his successor" diagnosticated malignant disease in the popliteal space of a patient, knowing nothing of his antecedents, and for this affection amputated through the thigh, subsequent examination proving that the diagnosis was wrong, as also the treatment, since no malignant disease existed.

I succeeded Prof. Chisolm as surgeon to the Baltimore Infirmary (University Hospital), consequently the quotations and the deduction refer to me. The statements contained in these quotations, and the inference to be drawn therefrom, are at variance with fact.

"Not having seen it" (the case) "before."

During the time that J. F. was in hospital, I was attending two cases of urethral stricture in the adjoining ward, and saw him frequently. I was present during one of Prof. Chisolm's visits to the ward, when he made some clinical remarks on the case of J. F. I was present at the meeting of the Baltimore Pathological Society, April 19, 1872, at which meeting Prof. Chisolm related the case of J. F. Thus it is apparent that I was thoroughly conversant with the history of

J. F., and saw him repeatedly while under treatment for aneurism in the popliteal space.

"Considered the fistulous opening and polished skin evidences of malignancy, and amputated the limb."

I took charge of the infirmary July 1st, relieving Prof. Chisolm. On making my first visit I saw J. F., who had returned for further treatment, not having been cured during his previous stay in hospital.

Recognizing him at once, I referred him to the surgeon under whose care he had been previously, and in whose charge he would have been had he entered hospital twenty-four hours sooner, Prof. Chisolm. I then continued through the wards. After my visit, Prof. Chisolm asked me to to see J. F. with him. I did so; I examined the case very carefully, expressed the diagnosis malignant tumor, and advised amputation. Prof. Chisolm adhered to the diagnosis aneurism, but also advised amputation. Dr. Christopher Johnston, Professor of Surgery in the University of Maryland, being in the house, I requested a consultation. He confirmed my diagnosis, and concurred in the treatment recommended.

The three surgeons, therefore, who saw the case, advised the same treatment, namely, amputation—Prof. Chisolm for aneurism of some branch of the popliteal artery incorporating the sciatic nerve in its wall; Prof. Johnston and Dr. McLane Tiffany for malignant tumor. I make use of Prof. Johnston's name in connection with this statement by his permission.

It being my term of service, Prof. Chisolm turned the case over to me for treatment. I declined to amputate for consolidated aneurism, and determined on an exploratory incision. I sent the patient into the amphitheatre, Profs. Johnston and Chisolm being present, I gave chloroform, put on a tourniquet, and laid the tumor freely open. I believed my opinion unmistakably confirmed by the appearance of the growth.

Profs. Johnston and Chisolm examined the tumor thus laid open. Again Prof. Johnston concurred in my diagnosis.

I amputated forthwith.

Thus it is plain that, though I did consider the fistulous opening and polished skin evidences of malignancy, I amputated only after I had cut open the tumor to examine it thoroughly, and after my diagnosis had been concurred in by the senior surgeon to the hospital, Prof. Johnston; the amputation being advocated by both Prof. Johnston and Prof. Chisolm.

In regard to the microscopic appearance, I find incontestable sign of malignant disease. Prof. Johnston finds the same, so he tells me.

I have submitted the tumor to Assistant-Surgeon J. J. Woodward, Army Medical Museum, at Washington.

I enclose a report from the resident physician of Baltimore Infirmary, as to the result of the case.

I have the honor to remain, with high regard, yours,

L. McLane Tiffany, B. A. (Cantab.), M. D.,
Demonstrator of Anatomy, University of Maryland.

J. F. was admitted to the Baltimore Infirmary on July 1st, in the service of Dr. Tiffany, with tumor in popliteal space, for treatment of which he entered the hospital. The tumor was diagnosed to be malignant, and amputation at the junction of the middle and upper thirds of the thigh was done at once by Dr. Tiffany.

The patient was discharged during a temporary absence from the city of the undersigned, but he has been informed by Dr. Norris, Assistant Physician, then in charge, that he left with a stump not entirely healed but nearly so, and in a healthy condition. Richard H. Lewis, M. D.,
Resident Physician.

Bibliographical and Literary Notes.

Art. I.—*Address before the American Medical Association, Philadelphia, May* 7, 1872. By David M. Yandell, M. D., President.

The above is the title of a very neat pamphlet printed by John P. Morton & Co., publishers, Louisville, Ky., and soon to appear in the "Transactions of the American Medical Association."

In every age and every country wherever civilization has found a home, the medical profession has held a foremost place in the republic of learning. Even in America, where many untoward circumstances have combined to lower and debase it, none can deny the high rank of its leading men in the "mighty dominion of genius and lore." With these the author of the pamphlet under notice is very justly classed. The American Medical Association showed its great appreciation of his merit by electing him to the highest office within its gift, at the session held in San Francisco in 1871, and the address delivered in Philadelphia of the present year proves that they made no mistake as to his ability, while it shows him to be a finished scholar as well as a learned and accomplished physician.

Dr. Yandell commences his address with an interesting historic account of some of the earlier pioneers and distinguished teachers of medicine in Philadelphia, that grand old cradle and nursery for medical men, as beautiful and charming as they are just and appropriate.

We listened to this part of the address (being present on the occasion of its delivery) with the deepest interest and greatest pleasure; and as we heard his chaste and graceful allusions to those olden men and times, we were forcibly reminded of our own *alma mater*, the once famous Transylvania, where we had, in our youth, so often listened to the silvery and eloquent tones of his honored father, as he invested even the dry details of chemistry with the poetry and interest of a romance. But when, a few seconds afterward, our author began to treat of the all-absorbing topic of medical education, with the memory of Rush, and Chapman, and Jackson, and many others, whose profound wisdom, deep learning, and brilliant lives, he so beautifully epitomized, still fresh upon him; and the example of that venerable sire in whose lap he was reared, and by whose graceful diction and winning eloquence his youth must have been inspired, still before him, it was difficult to understand how he could commit such an open revolt against the teachings of those noble old masters. And we could not repress the thought that for him at least they had lived in vain, and that it would be through no default of his that the graduates of the coming years do not forget or despise them as men of too much learning to suit the demands of the times.

His views on the subject are still more astonishing and unaccountable, when we remember the teachings of him whom he claims to have been the instructor of his early youth, his "honored master, Prof. Gross." Will not Dr. Yandell admit that, among the elements which have conspired to make Dr. Gross so justly famous throughout the civilized world as author and teacher, not the least were derived from his attainments in general science and literature, and that discipline of thought which general culture alone can give? It is perhaps fortunate for Dr. Yandell that his old preceptor entertained far different views of the necessary prerequisites and constituents of a medical education from those which his pupil has endeavored to inculcate.

Had the opinions expressed in this part of the address been delivered in a merely private capacity, they might merit only a derisive smile as a weak and specious plea for cheap medical schools; but, owing to the elevated position held by the author, they deserve to be exposed by the profession as a most flimsy apology for a system of teaching which is full of danger to the profession, and fraught with mischief to the American people.

Dr. Yandell acknowledges that the principal aim in the formation of the Association, and the object to accomplish which it has endeavored to use all its energies, is the advancement of medical education. He tells us that in the first convention which met in New York, in 1846, "reform was the watchword," and quotes from numerous authors to prove that from that time to the present it has never lost sight of its all-absorbing interest in this one grand purpose. As president of the Association, it was expected that he would aid in this movement for advancement, and, to say the least of it, it would have been much more becoming had he chosen a different time and place to arraign and condemn all the efforts hitherto made in that direction, and to promulgate such mischievous and opposite doctrines. That he found a medical periodical at a railway-station on the Rocky Mountains, and met there a doctor who knew how to give hydrate of chloral and bromide of potassa, seem to be among his strongest reasons for con-tending that the medical profession is sufficiently advanced, and possibly too much so, to meet the demands of the times for "rugged utility."

Philosophers and historians have long labored to convince the world that a stand-still and a retrograde were equivalent; but this Dr. Yandell does not believe, so firmly is he convinced that in American medical education no improvement is necessary. He tells us that "the idea of reform has been the idea underlying the whole movement, and animating its most active members with a zeal which could hardly have been inspired by a less generous cause;" that "beyond all controversy there are grave defects in the education of many of our students and many of our practitioners of medicine;" that "too many of them know little about etymology and are bad spellers," and then very complacently apologizes for these evils by asking if "matters are worse in this respect than they were in the time of our forefathers?"

Notwithstanding this has been the theme of the choicest productions of many of our best and wisest men, acting on committees annually appointed, and despite the earnest appeals of many of our most eminent teachers, Dr. Yandell in effect declares that they are only murmurers and croakers, always to be expected and never to be regarded. And after flippantly disposing of complaints from such high authority, supported by unimpeachable statistics, he would have us believe that no improvement is needed! Alas, that the renowned Dr. Pangloss cannot again visit the earth to rejoice with his worthy disciple! How gratifying it would be for the duet to embrace one another and proclaim in glad exultation that in American medical education, at least, whatever is is best—every thing is best.

The doctor sets out by condemning the proposition for national medical schools, but fails to give any good reason why they should not be adopted as a cure for the evils complained of. This plan has already been tested by many of the countries of Continental Europe, with a success which leaves no doubt of its value. And, while it must be confessed that the anomalous relationship which the States of this Union have to each other and to the General Government renders such a system open to serious political objections, yet the writer believes that a plan might be devised which would make it entirely independent of this disturbing and dreaded element. But, politics aside, there can be no reason against it not counterbalanced by many in its favor; and if this country should ever become a nation, the good sense of the law-makers and the interest of the profession will surely inaugurate a system of national medical schools in keeping with the spirit of the age.

Dr. Yandell then takes up the German schools of medicine, which, after a few words of unpardonable mockery and ridicule, he dismisses with a prayer that ours may never resemble them. He says the Germans cultivate medicine "with unwearied zeal and assiduity," and in the same breath he tells us they stand "idly by while disease is running its course, curiously marking its natural history, looking on calmly . . . . intent mainly on tracing its ravages in the cadaver, too well satisfied to find their diagnoses confirmed by the autopsy." This is, indeed, a remarkable assertion to be made before a large body of learned and scientific men. What must we think of the frankness and fairness of one who can so pervert fact and history in his desire to attribute such brutal, if not demoniac, natures to his brothers of Germany, who have done and are still doing so much for the advancement of the medical profession and the alleviation of human suffering, and whose only offence is that they demand a high grade of preparation from those who would be invested with the honors of the doctorate? Truly, the village limb-setter he so much exalts would make but a poor shift were it not for the labors of the profound students and inquirers whom he thus ridicules. His whole address develops a most violent antagonism to all trained thought and educated method, and he is bold enough to follow his theory to its legitimate end by contrasting the learned physician of Germany with the American cross-roads doctor, for the latter of whom he does not hesitate to declare his preference, and for whose "rugged utility" (?) he would exchange the cultivated method of the learned and accomplished physician.

While thus ridiculing the educated, trained, and finished surgeons of Europe, and their ample and wise curriculum, he gives his own "schedule," in which he says "clinical instruction should be the alpha and omega of a medical education." He would care nothing for "those 'vague and misty perceptions of fundamental medical truth' with which teachers complain of meeting occasionally in the green-room," but, without preliminary education and ignorant of the elements of medicine, he would require his pupil to spend most of his time at the bedside until he had made himself "master of his art." He would have him chop limbs and saw bones, and thus he would instruct him in those great "fundamental medical truths" and principles of pathology about as well as were the barber-surgeons and Dr. Sangrado of old, who had quite as much of clinics as Dr. Yandell could desire. Clinical instruction was their alpha and omega, yet he would scarcely be regarded a sane man who, at this day, would contend that their method was the right one. Dr. Yandell certainly trenches closely upon such a proposition when he warns us to beware how we permit German study and German thought to meddle with the art of medicine. His remark, *en passant*, that no school without ample means of clinical instruction can long sustain itself, is fully refuted by the medical department of the University of Virginia; for that institution, while its professors doubtless deplore the want of means for clinical instruction, is yet so elevated in its standard of requirements and the thoroughness of its course, that, in spite of this incurable drawback, its graduates rank with those of any school in the United States.

Dr. Yandell, in his defence of his proposition that the Association has "attached too much importance to this humble accomplishment of spelling" and a good English education, cites us to the example of the great lithotomist and surgeon, the late Dr. B. W. Dudley, and his colleague, Dr. W. H. Richardson, both at one time professors in the once-renowned old University of Transylvania. What he says of Dr. Dudley is, in our opinion, unjust and entirely without foundation. Though not a voluminous writer, Dr. Dudley was a fluent and graceful lecturer; and the writer, who sat with pleasure at his feet, has often been charmed with the impressiveness of his well-chosen words. What he says of Dr. Richardson is more nearly correct, except that he never was regarded as a good lecturer or successful teacher. He was a good man, but "history" has already recorded as much in his praise as a teacher as the facts will justify. It is probably true that, illiterate as he was, and without a thorough professional training, he was a good manipulator for one of his day and location; but will not our author admit that, had he not been denied the advantages of a thorough education, he would have understood much better the science and mechanism of obstetrics, and would have been more efficient both in his practical and didactic sphere, besides being saved the mortification he endured from "his grammatical inaccuracies, which often raised a smile on the countenances of his pupils?"

Another part of the address, which had much better been left unwritten, is that in which he seeks to enforce his argument against a high grade of education by a waggish and flippant condemnation of Dr. Daniel Drake's great work on "Medical Topography." Following the example of Brontius, Cleghorn, Bajon, Hillary, and other distinguished writers of Europe, this illustrious man has given to the world a volume on the "Diseases of the Interior Valley of North America," which in point of scope, arrangement, style, and the value of its contributions, far exceeds all other similar works combined, from the days of Prosper Alpin to the present time; and which is claimed by some of our best men to be a "contribution to medical science on this subject which stands without a parallel in medical literature." The tone of levity and disparagement with which our president treats this book is indeed a sad commentary on human gratitude, and a poor return for the labor, talent, wisdom, genius, and learning, of that master-spirit who has passed away after a life of virtuous ambition and faithful toil. It was to this work that he devoted the best energies of his life. It was for this that his stout heart was willing to brave the summer's heat and encounter the noxious vapors of the Mississippi Valley in order that he might investigate in person the hidden sources of disease in that pestilential region. And in doing this he has collected together a mass of facts so extended, so various, and so valuable, that it is doubtful if the lives of many men could be lengthened to a span which would make them equal to the task. It is well, perhaps, for Dr.

Drake that he has not lived to see the day when the work, on which he labored so zealously and faithfully, and which he considered the pearl of his great life, can be thus slightingly spoken of by our president, when in the presence of the assembled medical wisdom of the Union he sees fit to repeat from his high place, with apparent approval, the language of a "very intelligent relative" of his, a Tennessee doctor, who declared to him that he "would not read it through for a hundred dollars!" Dr. Yandell certainly shows himself familiar with the title-page, but whether or not he has ever read it himself he does not tell us. His objection to this book is, not that any portion of it is obscure, incorrect, unnecessary, or false, but that it is too far in advance of the age, and too large. This is, perhaps, the first time in the history of the world that a man engaged in teaching has found fault with a book because "it was in advance of the generation for which it was written." For the same reason he would have derided and scouted Bacon, Newton, Galileo, Harvey, and those great luminaries who have benefited mankind by their scientific discoveries, but in doing so had been somewhat in advance of the age of mere "rugged utility." If, indeed, his assertion is true that this book is "in advance of the generation for which it was written," and for this reason has been left quietly "on the shelf to collect dust and cobwebs," there could not be presented a more convincing proof of the absolute necessity for a speedy advancement in the standard of medical education.

Our author seems to regard medical schools as manufactories of doctors, which must regulate the quality of the article supplied by the demands of the market. The schools must be governed by the requirements of the people, for they know best what kind of doctors are needed. The public demands a doctor who can reduce a dislocation, and who is handy in the "rugged utilities" of the profession, and cares nothing for his learning or his science. The schools must hear and obey. They must lay aside all logic, all comprehensive treatises, all advanced ideas, in short, all things that cost time and money, and wait till another day before they attempt a thorough education. Dr. Yandell seems to entertain, and indeed expresses, a great antipathy for logic. Medical men, he thinks, should chop limbs, and let logic alone. And to show his sincerity in this, and his utter contempt for all the laws of logic, he indulges in the following beautiful syllogisms. The first specimen, the logic-choppers, to whom the doctor alludes, could not name; but the limb-choppers of his persuasion might call it an enthymeme beside which the famous syllogism of Descartes pales into insignificance:

1. Some men, despite the disadvantages of a want of education, have become useful and distinguished doctors of medicine.

Therefore, no man needs education, but all may become useful and distinguished doctors without it.

2. Dr. Richardson and Napoleon I. could not spell correctly.

Dr. Richardson was a successful physician, and Napoleon a successful general.

Therefore, all men who cannot spell correctly will become successful physicians or generals.

3. I predicted twenty-two years ago that "no school without an ample supply of subjects and adequate means for clinical instruction could long sustain itself."

I say now that time is fully justifying my prediction.

Therefore, "here, in the midst of these clinical demonstrations, physic is to be learned, and not by going to universities."

Any one who has read the address will admit that the above is a fair specimen of the doctor's dialectics, and it cannot be surprising that one entertaining such ideas should be disgusted with all kinds of logic.

Whatever may have been said of our system of education, no one ever dreamed it was quite so bad as Dr. Yandell's address has proved it to be. Of all the ills which might have been feared, no one supposed that it would so soon develop a champion of the empirics[1] in such a quarter. But still we cannot conceal from ourselves the fact that such has been the result, for Dr. Yandell only revives the doctrines of that long-reviled and derided sect, and in effect declares it is time to tear down the statues of Hippocrates, Galen, and Sydenham, and to elevate in their stead the brazen image of Paracelsus, who, somewhat in conformity with this modern doctrine of "rugged utility," boasted that he had not read a book in ten years, and on the occasion of his first lecture publicly burnt the works of Galen and Avicenna, declaring at the same time that the hair of his head contained more experience than all the academies in the world!

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[1] I use this word in its original and proper significance, and not in its common and offensive acceptation.

Dr. Yandell winds up his address by some very sensible and appropriate remarks on woman's right to enter the profession of medicine. His views on this subject are briefly expressed, but well considered, and such as are generally accepted by the profession.

In closing this notice of Dr. Yandell's address, we must say that we regret to be compelled to differ so widely from him on the subject of American medical education. Separate from the views declared in this direction, we are happy to repeat the expression of our great admiration for his well-written, chaste, and very beautiful address. The scholarly execution of this production shows most clearly that our author has not adhered to the curriculum under which he was trained when giving us the "schedule" which he would mark out for his pupils.

W. O. B.

ART. II.—*Lectures on the Principles and Practice of Physic. Delivered at King's College Hospital, London.* By Sir THOMAS WATSON, Bart., M. D., F. R. S., etc. In two volumes. From the fifth revised and enlarged English edition. Edited, with Additions and numerous Illustrations, by HENRY HARTSHORNE, A. M., M. D., Professor of Hygiene in the University of Pennsylvania. Philadelphia: Henry C. Lea, 1872.

A REVIEW of Watson's "Practice" is to-day scarcely possible. We cannot sit down to weigh seriously the merits and demerits of a book which has for twenty years occupied a familiar place in the library of almost all English and American medical men. From it the younger of us for years drew much of our instruction, and the older have been, in the words of the editor, "refreshed if not informed by its luminous pages." Of course, written as it was, at a time when medical science was growing with wonderful vigor, the book could not long keep abreast of the times in all respects; but it would long, and we think always, be consulted with pleasure, if for no other purpose than to find how clearly and pleasantly the older and more definitely-settled truths of our art can be expressed. Its easy, running sentences and graceful periods were always our defence when the barbarous "style medical" was ridiculed as it justly deserved.

We confess to have always had, toward any one who should attempt to add to the book, a feeling akin to that which we experience when we see an old picture "restored," or a picturesque or historical house modernized. But if we throw aside our associations, and examine this edition upon its merits as compared with those of the former, we shall find really an advance. The bulk of the matter stands as in the former edition, although fourteen years have elapsed since its publication; and this is well, for, while twenty years make a difference in the reader, they make far less in disease or the essentials of its treatment. The chief changes of the original matter are in the consideration of diseases recognized or become prominent since the issue of the previous edition. Changes, too, have been made in accordance with more recent investigations upon maladies which had already been discussed at length. Thus we find some change regarding cancer; regarding diseases of the nervous system, especially those more recently recognized, as locomotor ataxia. Diphtheria is discussed in this edition, and additions are made on the treatment of ovarian dropsy, with reference mainly to its surgical cure; on the essential nature of cholera; on kidney-diseases, and on fevers.

Change, too, is observable in the discussion of the value of bloodletting and mercury as remedial agents.

These changes, however, while enhancing the value of the work, have not increased its real size. The new matter has been substituted rather than appended—we are comparing with the previous American edition—and the omission of the four chapters on diseases of the eye, as a subject now no longer within the scope of a work on general medicine, has given space for the changes. The form of the book has been improved by enlarging the type, so that the page contains now about two-thirds of a page of the last edition, and by the dividing the work into two more handy volumes. In the American edition the greater part, certainly the more important part, of Dr. Condie's additions have been retained, and a few emendatory articles have been inserted by Dr. Hartshorne.

Despite the number of recent able works that have appeared on practice, we doubt not there will still be a demand for this old favorite.

Art. III.—*Ovarian Tumors; their Pathology, Diagnosis, and Treatment, especially by Ovariotomy.* By E. Randolph Peaslee, M. D., LL. D., Professor of Gynæcology in the Medical Department of Dartmouth College, etc. New York: D. Appleton & Co., 1872. 8vo, pp. xxxii.-550.

Although a great deal has been written within the past twenty years on the subject of ovarian tumors and their treatment, and especially of ovariotomy, we believe that the work of Dr. Peaslee is the first complete monograph on the subject that has yet appeared in any language. In the words of the preface, "while several writers have published their individual experience, more or less extensive, as ovariotomists, no work has appeared of broader scope which proposes to cover the whole ground so far as is practicable within the limits of a single volume." The book is not, and does not purport to be, a treatise on all the diseases of the ovary, but deals only with ovarian tumors and their treatment. It is divided into two parts, the first including the normal anatomy of the ovary, and the pathological anatomy, the pathology, diagnosis, and treatment of ovarian tumors, excepting by ovariotomy; and the second treating of ovariotomy alone, including its history, statistics, practical details, and after-treatment.

The chapter on the normal anatomy of the ovary, which is the first in the book, is essential for the proper appreciation of what follows, and is as interesting as it is instructive. It contains all that is known on the subject and much that is comparatively new. Chapter II. is devoted to the classification and pathological anatomy of ovarian tumors, which are primarily divided into solid and cystic tumors. Of the five varieties of solid tumor enumerated, only two, fibroma and carcinoma, ever attain a size calling for the interference of the surgeon, and these are considered as fully as their importance demands.

Carcinoma of the ovary is believed by the author to be much more rare than is generally stated. He has himself "seen but two cases of demonstrated and two of strongly-suspected cancer commencing in the ovary," and he refers to the experience of T. Spencer Wells, who met with only three cases of cancer in his first four hundred operations; and of Dr. Charles Clay, who found but six instances of undoubted carcinoma in five thousand cases diagnosticated by him.

As this is not simply a question of pathological interest, but one of great practical importance, both as regards diagnosis and treatment, it is very desirable to know whether cancer of the ovary is really as rare as the experience of the author would indicate, or whether that experience is not itself exceptional. In an experience certainly not larger than that of the author, Prof. T. G. Thomas has met with no less than nine undoubted cases of ovarian cancer, and we have ourselves met with no less than seven instances of demonstrated cancer of the ovary in a considerably smaller number of cases than has come under the observation of those gentlemen. If, as is stated by the author in another place (page 106), "uterine fibro-cysts are far more rare in Europe than in this country," may not the same be true of cancer, and may we not thus account for the discrepancy between operators here and abroad, for we are not aware of any other American operator whose experience in this respect fully coincides with that of Dr. Peaslee. However this may be, we entirely agree with the author in the following opinion, as stated on a subsequent page (p. 179): "In doubtful cases it is our duty to assume that carcinoma does not exist until we can be very certain it does exist from symptoms which are to us convincing; or, if such signs are not present, to regard the case as non-malignant, and proceed to the operation till it is proved to be malignant by ocular inspection. . . . The useless resort to ovariotomy in a case of carcinoma results in a closure of the incision on discovering the real nature of the case, and a possible, but not very probable, fatal result in a case that must soon prove fatal at any rate. The other mistake dooms to death a patient who by the operation has the average chance to be saved."

Fibroma of the ovary occurs, the author thinks, about as often as carcinoma, he having met with two cases himself. Here, too, we are inclined to regard the author's experience as exceptional, and believe fibroma to be the rarest form of ovarian tumor. Indeed, so rare is it, that many pathologists have been led to doubt whether an unquestionable case has ever been seen.

Of cystic tumors of the ovary the author makes three classes, including five varieties. Two of these varieties, hydrops folliculorum and struma ovarii, inasmuch as they never attain to any considerable size nor require surgical treatment, are disposed of in a few sentences. The other three varieties, oligocysts, polycysts, and dermoid cysts, are those which almost exclusively engage the services of the ovariotomist. After a very interesting section on the general structure and contents of ovarian cysts, we come to a description of the mode of origin and pathological peculiarities of each of these three forms. The term oligocyst is preferred to monocyst because it designates a class of which the latter is but one variety, and includes all the forms that originate in the same way and possess the same general characteristics. For example, the so-called monocystic tumors are always developed from the ovisacs, and may be monocystic from the first (though Rindfleisch holds a different opinion), or may become so from the coalescence of several smaller cysts. But, as there is only a limited number of ovisacs engaged in the production of the tumor in the first place, there must be a corresponding limitation to the number of cysts that can possibly be developed.

These may entirely coalesce and form one cyst, or partially coalesce and form several, but the number is always limited, and hence the term oligocyst, which expresses this limitation.

The polycystic tumor originates, not in the ovisacs, but in a colloid degeneration of the stroma of the ovary, as is very clearly shown by Rindfleisch, and may develop cysts indefinitely.

The mode of origin of the dermoid cyst, which is characterized by the presence of hair, teeth, and bone, is still unexplained. The various theories that have been advanced concerning it are fully discussed, however, in the third chapter, which treats of the growth, duration, causes, and symptoms of ovarian tumors.

The fourth chapter deals with the complications with ovarian cysts, and other enlargements often mistaken for them, the principal complications being pregnancy, ascites, uterine fibroma, and a second ovarian cyst.

Of the enlargements that may be mistaken for an ovarian cyst, the only ones considered in this chapter are cysts of the broad ligament, hydrosalpinx, and uterine fibro-cystoma. Respecting the last of these we are glad to find the author making the following distinction: "Their name suggests a secondary cyst developed in a uterine fibroid. This is, however, not always the fact; since often no truly fibroid element is met with in the tumor, there being merely a cyst in continuity with the uterus, whose thick vascular wall is formed like the latter of non-striated muscular fibres and connective tissue."

Chapter V. treats of the diagnosis of ovarian tumors as based upon their general and local signs. In the words of the text: "The *general signs* include the history of the case, the expression of countenance, the appearance of the neck and upper extremities, the degree of activity of the kidneys, and sympathetic affections of the mammæ. . . . The *physical local signs* are obtained by inspection, mensuration, palpation, percussion, auscultation, and change of patient's position; from the vaginal touch, the use of the uterine sound, the rectal touch, the exploring trocar; from chemical and microscopical examination of the fluids obtained; and from exploratory incisions." Every one of these points, except the exploratory incision, which is reserved for a later chapter, is treated with a fulness and clearness that leave nothing to be desired. In the next chapter the question of differential diagnosis is discussed with that completeness that is so characteristic of the author; and this and the preceding chapter are perhaps the two most valuable in the book, dealing as they do in the most satisfactory manner with the most difficult problem connected with ovarian tumors.

No less than twenty-four different pathological conditions are enumerated in this chapter, that have been mistaken for ovarian tumors, "and many of them very often;" and, as the author truly remarks, "even this list might be increased." The chapter concludes with the differential diagnosis of the three varieties of ovarian cyst from each other, which is not, as might at first be supposed, a mere refinement of diagnosis, but is a matter of considerable practical importance, for each of these forms has certain tendencies peculiar to itself that have an important bearing on prognosis and treatment. In Chapter VII. the diagnosis of adhesions, of the length of the pedicle, and of carcinoma, and the value of tapping as an aid to diagnosis, are discussed in an able and exhaustive manner. Chapter VIII. treats of the prognosis of ovarian cysts when left to Nature or merely palliative treatment, and of the method of taking notes of a supposed case; and Chapter XI., which concludes the first part of the book, is occupied with the treatment of ovarian cysts, excepting ovariotomy. The first five pages of this last chapter are devoted to the medical treatment of ovarian cysts, the efficacy (or inefficacy) of which is expressed in the concluding sentence: "Internal medication, therefore, at most, merely sustains the patient's health for a time, and thus somewhat postpones the fatal result." The question of simple tapping as a *palliative* procedure is then fully discussed, and the manner of its performance, its dangers, and the class of cases in which it is admissible, pointed out. We then come to the consideration of tapping as a *curative* measure; and the results of tapping followed by pressure, tapping followed by iodine injections, and tapping followed by the maintenance of a permanent opening into the cyst, are discussed at considerable length, more especially the last two.

This chapter, which is one of the fullest in the book, is no less remarkable for the great research which it evidences, than for its searching analysis and cogent reasoning. As a negative argument in favor of ovariotomy it is unanswerable, the conclusion being inevitable that, for the great majority of cases of ovarian cyst, ovariotomy is the only remedy.

The second part of the book is devoted entirely to ovariotomy—its history, statistics, indications, prognosis, operative methods, and after-treatment. Nearly one hundred pages are occupied with the history of the operation, and this part of the work has evidently been a labor of love with the author, for only one thoroughly enamoured of his subject could ever have undertaken the immense labor of collecting and verifying the vast amount of historical detail contained in this chapter. It is probably destined to be not only a monument to the industry and accuracy of its author, but also the principal source from which all future writers on this subject will draw their supplies. After a careful analysis of the earliest reputed cases of ovariotomy, and after proving beyond all question that the credit of originating the operation belongs to Dr. McDowell, of Kentucky, he proceeds to give the history of ovariotomy in the United States, in Great Britain, in France, in Germany, and in the other countries of Europe and Asia, beginning with the earliest recorded case in each country, and ending with the year 1871. Regarded merely as a chapter in the history of medical science, it is one of the most interesting that has ever been written.

The statistics of ovariotomy, the objections that have been urged against it, the considerations in its favor, and the classes of cases to which it is adapted, are considered in the next chapter, and in the same thorough manner.

In Chapter III. the author discusses, at length, one of the most important questions connected with ovariotomy, viz.: "Should the operation be performed while the patient is in full general health, or be deferred until the latter is somewhat impaired?" As is well known, distinguished ovariotomists hold directly opposite opinions on this point. The author concludes a very careful argument of the subject with this statement: "As a general rule, therefore, I conclude that, *when the general health has become somewhat impaired, and not till then, the time for ovariotomy has arrived.*"

Chapter IV. deals with the principal conditions predisposing to a favorable or to an unfavorable result of ovariotomy.

These are divided into five principal classes, viz.:

1. Conditions cognizable before the operation.
2. Those affecting the operator himself.
3. Those detected or produced during the operation.
4. Those occurring after it.
5. The influence of hospital practice.

Each of these has numerous subdivisions, there being no less than thirteen under the first head, and each receives the most careful consideration.

In Chapter V., the indications and contraindications for ovariotomy are detailed, as also the conditions that should lead to an abandonment of the operation. The following opinion of the author, on a point of great importance to operators, will be read with interest, and will, we think, command general assent:

"But shall we absolutely decline to operate in all of the very unfavorable cases? Certainly not. If the conditions are not such as absolutely to forbid the operation, and the patient, after fully understanding the danger of the operation, entreats us to give her the slight chance there may be of thus prolonging her life, we are not at liberty to refuse to operate, this being done with a full understanding, on the part of all interested, on what grounds it is undertaken.

"Thus we shall not have to regret our action, even if we fail; while we will sometimes have the intense happiness of having saved one who was ready to perish."

T. Spencer Wells takes precisely the same ground. The treatment and arrangements preparatory for ovariotomy, and a description of the operation with general remarks upon it, form the subject of the seventh chapter; while, in the eighth, the history and practical details of the several stages of the operation are set forth. These chapters are very full, and exceedingly interesting, and readjust many claims to priority.

Not less than twelve pages are devoted to the primary incision alone, and yet there is not a sentence that could well be omitted. The comparative merits and disadvantages of the long and short incision are fully considered, and some account of the controversy on this subject among the earlier English operators is given. The methods of exploring for adhesions, of evacuating the cyst, of managing adhesions, and of extracting the cyst, occupy the next twelve pages. We then come to the treatment of the pedicle, and the full and exhaustive manner in which it is discussed may be inferred from the fact that it occupies nearly forty pages.

The history of every method that has ever been devised and recorded is here given, with the name of its originator, and the circumstances under which it was resorted to, and many new methods are thus shown to be comparatively old. For example, the method proposed by Dr. H. R. Storer, and called by him "pocketing the pedicle," is shown to have been first used by Langenbeck, in 1851. The use of the clamp, commonly ascribed to Mr. Wells, originated with Mr. J. Hutchinson, of London, etc., etc. The author himself prefers the ligature to the clamp. He ties the pedicle in segments, and, after cutting the ends of the ligature short, allows it to drop back into the pelvic cavity. He has devised a very ingenious method by which the ligature may be cut and withdrawn at the expiration of any given time without opening the wound, thus obviating the theoretical objection to leaving any foreign substance within the pelvic cavity. It has been successfully tested in one case, and is fully described in the text. His preference for the ligature, however, does not amount to a prejudice, for, after giving the grounds of his choice, he ends with the following statement: "Since all the preceding methods of treating the pedicle have given a fair degree of success, it must be inferred that success depends far more upon the general condition of the patient, and other points not at all connected with the pedicle, than upon the one I have been considering.

"Each method now most in use may have its advantages in particular cases, and the surgeon should be prepared to adopt the one he judges best in each instance." The examination of the other ovary, the cleansing of the peritoneal cavity, the closure of the incision, and the dressings, occupy the rest of the chapter.

In the eighth chapter, the treatment after ovariotomy is fully discussed.

The importance which the author attaches to this is shown in the circumstance that, until quite recently, he has declined to operate in any case where it was not convenient for him to have the subsequent care of the patient.

"The surgeon," he says, "who takes charge of a case after an ovariotomy performed by another, assumes much of the responsibility of the case, and four-fifths at least of the anxiety," and this statement we can fully confirm.

The *general* treatment of the case is first considered, and then that of the special symptoms and conditions that are liable to occur, such as vomiting, collapse, hæmorrhage, acute peritonitis, asthenic peritonitis, septicæmia, etc.

Each of these is fully considered, and the use of intra-peritoneal injections for septicæmia, first proposed and practised by the author, is described at length. The ninth and last chapter is devoted to the hygienic condition of those who survive ovariotomy, and to the *post-mortem* appearances in fatal cases.

Not the least interesting part of this chapter is that which treats of the effect of the removal of one ovary upon the reproductive powers of the woman.

It is shown that after ovariotomy a woman has not only borne children of both sexes, but has also given birth to twins. That the removal of both ovaries does not necessarily or even commonly render a woman less feminine in voice, appearance, or manner, is shown by the histories of many cases, and the judgment of the author on this point is given in the following statement:

"It cannot, therefore, be stated, as a general proposition, that the removal of the ovaries produces a change in the physical organization of woman, so as to make her more masculine in appearance, voice, and form; though such changes sometimes follow double ovariotomy as well as attend the progress of ovarian disease in cases not submitted to that operation; but they also occur independently of any suspected ovarian lesion." Very curious are those cases of alleged menstruation after the removal of both ovaries, in which the flow has recurred, not once merely, but many times, and at regular monthly intervals. The histories of six of these are given, and the nature of the phenomenon is ably discussed.

The chapter ends with an interesting section on the *post-mortem* appearances found in fatal cases. There is appended a short chapter on the bibliography of the subject, which is as complete as all the other chapters in the book.

In conclusion, we would say that, if we were to select a single word to express what we regard as the highest excellence of this book, it would be its *thoroughness.*

No point is left untouched that merits attention, and every topic is treated exhaustively, but without the least prolixity.

The general arrangement of the book is excellent, and the style is always clear and flowing. It is eminently a *readable* book, and is no less creditable to American medical literature than to its distinguished author.

ART. IV.—*On Food: its Varieties, Chemical Composition, Nutritive Value, etc., being the Substance of Four Canton Lectures.* By H. LETHEBY, M. B., M. A., Ph. D., etc., Professor of Chemistry in the College of the London Hospital, etc. Second edition. New York: William Wood & Co., 1872, pp. xv.–255.

THE readable little work of Dr. Letheby on Food has met with considerable success in this country as well as in Great Britain, and the second edition is much improved in matter and style. While the author has not aimed to produce an authoritative and thorough treatise on alimentation, he has certainly considered the physiological relations of food in such a way as to render the subject inviting and easy of comprehension to the better class of non-professional readers. Those parts of the work devoted to the history of different articles of food, their preservation, culinary preparation, etc., are peculiarly attractive from a pleasing vivacity of style as well as the valuable information compressed into a small space; and from this point of view the book has every thing to commend. It is unfortunate, however, that the average popular reader demands so much that is sensational in his semi-scientific recreations. There must be a demand for sensational science, or the supply would not be so abundant. In the little book before us, we find the common fault of statement of striking views as accepted facts, while they really rest on a most uncertain basis. Far better is it to present the old table of Beaumont, giving the time required for the "chymification" of different articles of diet, than to teach the public unsupported and unstable doctrines. Dr. Letheby gives Beaumont's table, which is good enough as far as it goes, but which was constructed when our knowledge of digestion was in its infancy, and does not represent the time required for the complete digestion of different articles; but it certainly is not possible, in the present state of science, to estimate the thermotic power, the labor-equivalents, etc., of different kinds of food.

As a compendious work for popular reading, there is much to praise and little to criticise in the book before us; but the reader should appreciate the fact that the production of force from matter cannot as yet be followed out and estimated with unerring accuracy, in such a complex machine as the human organism, and take some of the author's elaborate tabular statements with many grains of allowance. There is an amusing degree of *naïve* ignorance in the statement, under the head of Indian corn, that "the sallow, weazen look of the natives of the Northern States of America is thought to be due to the indigestible preparations of Indian corn, called mush, hominy, or Johnny-cake, which constitute the chief portion of their daily meals" (page 17). It is to be hoped that the account of English dietetics has more of a scientific accuracy.

ART. V.—*A Treatise on Localized Electrization, and its Application to Pathology and Therapeutics.* By Dr. G. B. DUCHENNE. Translated from the Third Edition of the Original. By HERBERT TIBBETTS, M. D. Philadelphia: Lindsay & Blakiston, 1871.

THOSE who have given any especial attention to electrotherapeutics are of course familiar with the extended work of Dr. Duchenne. The book is of great value, albeit the author carries his advocacy of faradization to the borders of bigotry. We should not look to him for a just weighing of the relative values, as therapeutic agents, of the galvanic and faradic currents. Abating this partisanship, the book is certainly a very desirable acquisition for the physician's library. The present part of the translation represents so much of the third edition of the work as had been printed before the investment of Paris, in 1870. It is mainly devoted to descriptions of currents, of batteries, and of instruments, with a general discussion, more or less polemic, of the therapeutical and physiological effects of the different forms of electricity. The consideration of especial disorders remains for its following parts. The translation is well done, and the notes which the translator has added are useful. The mechanical execution of the work is excellent.

ART. VI.—*Lectures on the Clinical Uses of Electricity, delivered in University College Hospital.* By J. RUSSELL REYNOLDS, M. D., Professor of the Principles and Practice of Medicine in University College, etc. Philadelphia: Lindsay & Blakiston, 1872, pp. 112.

DR. REYNOLDS's little book will, we think, more nearly meet the wants of the general practitioner than any other book we are familiar with. Not because it by any means replaces the larger treatises on medical electricity, but because it presents, in a very brief and comprehensible form, those details which are most constantly inquired about, and the search for which in elaborate treatises is tedious and often unsatisfactory. Of course those very familiar with this branch of therapeutics have no need of manuals, and those designing to make a thorough study will seek farther; but, while electricity in some form must be constantly applied by nearly every practitioner, a little work of concise information like this cannot fail to prove valuable.

ART. VII.—*Clinical Charts.* By W. W. KEEN, M. D. Published by Turner Hamilton, 106 South Tenth Street, Philadelphia.

THERE are two forms of these charts, whose design, as stated by the author, consists in aiding the surgeon and physician to record certain details in the physical examination of patients.

One form of these charts represents four diagrams of the different aspects of the body, two lateral, one anterior, and one posterior, on which physicians may inscribe, by certain abbreviations, the condition of the organs as resulting from physical inspection, auscultation, and percussion. The object of the diagrams is to avoid long verbal descriptions, as, for example, supposing he wish to record coarse *râles* in a certain portion of the lung, he makes a sign ‡ in that portion of the diagram corresponding to the portion of the chest over which he can hear these *râles*, and by a circumscribed line he can limit their extent. To represent dulness on percussion, he can shade a corresponding portion of the diagram. There are also nine other diagrams of the head and extremities upon which the surgeon can represent fractures of bones, both in direction and extent, tumors, or other deformities. These diagrams are printed upon one sheet of paper, folded together so that it can fit into an ordinary-sized envelop, each is numbered, and has a blank for date, name, and disease.

Another form of these charts may be used to represent variations in temperature to one-fifth of a degree, ranging from 94° to 108° Fahr., in pulse from 30 to 170 beats, and in respiration from 5 to 75 times a minute. It is obvious that such charts can be very serviceable in hospital practice and in clinical observation by every practitioner. The arrangement of the second form of chart is much better than the English devised and published by Hawksley, of London; we wish we could say as much for the paper upon which they are printed, which is of very inferior quality. R. A.

ART. VIII.—*Medical Thermometry and Human Temperature.* By C. A. WUNDERLICH, Professor of Clinic at the University of Leipsic, etc., and EDWARD SEGUIN, M. D. New York: William Wood & Co., 27 Great Jones Street, 1871.

THE first part of this book consists of an abridgment, by Dr. Seguin, of Wunderlich's celebrated treatise. In preparing it, Dr. Seguin has selected the points of greatest practical value, and has given us the result of observations on some thirty or forty diseases characterized by a rise of temperature; many of these chapters are illustrated by diagrams.

The second part contains "Suggestions on Thermometry and Human Temperature," by Edward Seguin, M. D., many of which suggestions will prove of value. Appendix I. will be found a great convenience by those not accustomed to reading any but the Fahrenheit scale. The table gives the equivalent in Fahrenheit, Reaumur, and in Seguin's medical scale of each tenth of a degree Centigrade from 33° to 45° C.—(91.40° to 113° Fahr.).

ART. IX.—*Headaches: their Cause and Cure.* By HENRY G. WRIGHT, M. D., etc., etc. From the fourth London edition. Philadelphia: Lindsay & Blakiston, 1871.

THIS practical little book has been long known to the profession. The new edition is but slightly changed. It will be found useful to us as a help to the management of this common but often intractable form of suffering.

BOOKS AND PAMPHLETS RECEIVED.—The Retrospect of Medicine: being a Half-yearly Journal, containing a Retrospective View of Every Discovery and Practical Improvement in the Medical Sciences. Edited by W. Braithwaite, M. D., and James Braithwaite, M. D. Vol. lxv., January -June, 1872. London: Simpkin, Marshall & Co.

Medical Electricity: A Manual for Students, showing the most Scientific and Rational Application to all Forms of Acute and Chronic Disease of the Different Combinations of Electricity, Galvanism, Electro-Magnetism, Magneto-Electricity, and Human Magnetism. By William White, M. D. 12mo, pp. 203. New York: S. R. Wells, 1872.

Thermic Fever, or Sunstroke. By H. C. Wood, Jr., M. D., Professor of Medical Botany, and Clinical Lecturer on Diseases of the Nervous System in the University of Pennsylvania, etc. Boylston Prize Essay. Philadelphia: J. B. Lippincott & Co., 1872.

The Principles and Practice of Surgery. By Frank Hastings Hamilton, A. M., M. D., LL. D., Professor of Practice of Surgery, etc., Bellevue Hospital Medical College, etc., etc. W. Wood & Co., 1872.

Small-Pox and Vaccination: the Predisposing Conditions of Small-pox and their Prevention. With a Scientific Exposition of Vaccination. By Dr. Carl Both. Boston: Alexander Moore, 1872.

Catalogue and Announcement of the Medical Department of the University of Pennsylvania, for the One Hundred and Seventh Session, 1872–'73. Philadelphia, 1872.

Medico-Legal Science. By Thad. M. Stevens, M. D. Report from the Transactions of the Indiana State Medical Association. Indianapolis, 1872.

On Chloroform. By Charles Kidd, M. D., Member of the Royal College of Surgeons, England, etc. London: Renshaw, 1872.

Transactions of the State Medical Society of Michigan, for the Year 1872. Lansing: W. S. George & Co., 1872.

Reports on the Progress of Medicine.

OBSTETRICS AND DISEASES OF WOMEN.

1.—*Inversion of Uterus reduced after Twenty-two Years' Duration.* By Prof. JAMES P. WHITE. Reported by A. T. LIVINGSTON. [Buffalo Medical and Surgical Journal, August, 1872.]

MRS. AIME DUBOIS, then aged twenty-four, and residing in Buffalo, gave birth to a female child, July 15, 1850. The midwife, who attended her in her confinement, experienced some difficulty in removing the placenta, but finally succeeded in *pulling it away*. This act was followed by so great hæmorrhage as nearly to destroy the woman's life. From this time until the day of the operation she suffered a continual loss of blood, which, twice a month, amounted to a "flooding."

March 2, 1870, Dr. Strong was called to attend Mrs. Dubois. He "found her lying upon a bed, about which were evidences in abundance of the terrible flooding that had just recurred." Upon making a digital examination, he discovered "a tumor about the size and shape of a small pear occupying the vagina," which, after a careful specular examination, he "diagnosed as an inverted uterus."

On June 23, 1872, on the invitation of Dr. Strong, Prof. White, with Profs. Julius F. Miner and M. G. Potter, Drs. George N. Burwell and W. C. Phelps, and the writer, proceeded to the residence of the patient in the town of Colden, Erie County, N. Y. She was found to be feeble and very anæmic, and slight hæmorrhage from the tumor was then occurring. Prof. Potter, who was requested to take charge of the anæsthetic, administered some chloroform to the patient, and the tumor was then examined by several of the gentlemen present. It resembled in size and shape an ordinary hen's-egg, and was suspended in the vagina by a long, narrow pedicle continuous with its smaller extremity.

This appearance, particularly the small and elongated cervix, led some who examined it to doubt its being a uterus at all, and to consider it rather a polypoid growth. But a probe could not be passed along the pedicle into the os, as might have been done had the tumor been a polypus.

The uterus could not be detected by palpation over the abdomen. A probe passed up the vagina was felt both by the finger in the rectum and the hand placed over the hypogastrium; also the finger passed up the rectum came in contact with the anterior abdominal wall as felt by the other hand; all these diagnostic means proving the absence of the uterus from the situation which it normally occupies.

By these negative proofs Dr. White was entirely convinced that the tumor was the inverted uterus, and he therefore proceeded to attempt the reposition of the same, the patient being kept under the influence of ether.

Dr. White now assumed the kneeling posture in front of the patient, who had been placed upon the bed so that her hips projected a little beyond its edge, her feet resting in the laps of Drs. Miner and Phelps, who sat upon either side of Dr. White, each supporting a knee and holding a hand of the patient. He then introduced his right hand into the vagina and began manipulating the tumor. This manipulation consisted in compressing the uterus, which relieved its congestion and rendered it more pliable, and in making gentle pressure in the line of the axis of the pelvis by use of the uterine repositor. After continuing this a short time, the doctor brought the tumor down to view, when a glance sufficed to assure the *doubting* of its true nature. By the pressure which had been exerted, the neck had been shortened and dilated, the body and fundus reduced in size, the superior angles (now inferior) were distinctly seen, and altogether the tumor then presented the normal outline of a uterus of small size.

The operator's uterine repositor consists of a stem of wood or hard rubber, about ten inches in length, straight or curved, one extremity of which is enlarged and cup-shaped to fit the fundus uteri, the other extremity having attached to it a coil of steel spring wire, against which the breast of the operator may be placed, who, by leaning forward, may exert a constant and gentle pressure upon the uterus, and thus relieve his other hand, with which he can facilitate the repositing by manipulating the upper end of the uterus, either through the abdominal wall or by passing a finger up the rectum. This repositor was again introduced, and a pressure of eight or ten pounds exerted by it, and, at the same time, compression was made by the hand within the vagina upon the portion of the uterus protruding beyond the os, the same hand also retaining the fundus uteri and cup of the repositor in coaptation, the left hand being employed as above suggested. When, in this manner, the cervix had been made to embrace the fundus, the uterine repositor was substituted by a large rectal bougie, with which pressure was continued until the close of the operation.

At the end of an hour and fourteen minutes Dr. White was obliged to discontinue his efforts on account of the benumbed condition of his hand, caused by the pressure upon it of the narrow and unyielding vagina.

The fundus, at this time, was within the cervix and above the os; and the doctor considered the reduction substantially accomplished. He requested Dr. Miner to continue the manipulation, which he did, and in sixteen minutes, or just one hour and a half from the beginning of the operation, Dr. Miner enjoyed the satisfaction of announcing that the uterus, which for twenty-two years had been completely inverted, was now as completely reposited.

Two hours after the operation, when we left her, the patient was quite as comfortable as had been anticipated. An opiate was administered, and directions were given the attendants to keep her perfectly quiet for at least a fortnight.

By letter, dated June 27th, Dr. Strong reports Mrs. D. doing better than he had dared to expect. July 2d, Dr. Strong writes: "Our patient is doing well, and nothing has yet interfered with her progress toward recovery. Pulse to-day, 68. Treatment is the same as reported last, except quinine, which was withdrawn on account of gastric irritability." The following letter, dated July 19th, which is the latest received from Dr. Strong, presents a most satisfactory conclusion to the report:

"Since the operation upon Mrs. D. nothing has transpired to mar the beauty of the result. The pulse has at no time exceeded 90 per minute. In two weeks from the day of the operation she began to sit in an easy chair, and since that time she has been more or less about her room. On the 14th instant an examination showed the uterus occupying its normal position, the intra-vaginal portion of the neck presenting the ordinary appearances of mild cervicitis. The patient is now about upon her feet some portion of her time, and every thing points to a rapid and, I think, perfect cure. She has had no sanguineous discharges since the operation. Pulse is now normal and appetite fair."

2.—*A Case of Placenta Prævia, with Twins.* By E. M. Wight, M. D., Chattanooga, Tenn. [Nashville Journal of Medicine and Surgery, September, 1872.]

On the 24th of April, I was called upon by Mr. S., who stated that his wife was "flooding considerably," and was expecting confinement in about three weeks. Upon close inquiry, I thought the flow not sufficient to demand immediate interference, and advised entire quiet; and inasmuch as she had twice before, within the month previous, had slight hæmorrhage, which the husband thought had been relieved by alum-water injections, I counselled their continuance, directing him to send for me very early when the labor should commence, and to send at once in case the hæmorrhage continued. The family living some miles out of the city, I heard nothing more of the case until the night of the 19th of the following month, when I was sent for hastily, with the word that Mrs. S. was in labor, and flooding.

Fearing the nature of the case, I made haste to the bedside, and found, on arrival, that the flow was not alarming, although sufficient to demand an immediate examination. The patient, who was possessed of an unusually clear understanding of such matters, stated at once that she feared that the flow was from the "after-birth," and that she was carrying two children. I did not feel authorized to dispute the correctness of this intelligent diagnosis, for, upon examination, I found the os but slightly dilated, and completely filled with what was, without doubt, the vascular substance of the placenta.

The peculiar shape and feel of the abdominal tumor likewise led me to believe that she was right in her opinion concerning the number of children. This was the third confinement—one eight and one five years before, both boys. During the early part of this pregnancy she had accompanied her mother, who was in very feeble health, to a watering-place in Michigan, and had been her constant companion, assisting her often by the employment of all her own strength.

The uterine contractions were but slight, occurring about every hour, and ceasing altogether before morning. There was no recurrence of the hæmorrhage, and no further dilatation of the os that day. This state of things continued almost *in statu quo* for nine days, with two slight recurrences of the flow and attending slight dilatation, at which time the uterine contraction assumed a more gratifying force, and the hæmorrhage returned so freely as to demand prompt action. The os was not yet of sufficient size to admit more than two fingers, and I was about deciding to plug the vagina and wait for more dilatation, when, upon examination, I discovered that the placenta border could be felt as far up as the finger could command, on the left side, and at once detached all that portion of the placenta on the left side of the uterus, as far back, posteriorly, anteriorly, and on the right side, as I was able.

During this operation the hæmorrhage was, of course, considerable, but, using the hand as much as possible for a plug, I think I was able to so press upon the ruptured placental vessels as to suppress the flow in a great measure. The head was presenting. After their detachment was completed, there was no further hæmorrhage worth mention, until the first child was delivered.

While the operation was proceeding, and during a few minutes after, the os had so far dilated as to admit of the introduction of the hand; but there being no hæmorrhage, and a vigorous pain taking place every four to seven minutes, I thought best not to further interfere. A few pains soon brought the detached portion of the placenta down through the os, accompanied by a good-sized protrusion of waters, which I soon after ruptured, though the membranes were the toughest I ever met with.

The first child, a female, weighing six and a half pounds, was delivered a few minutes after. The mother at once inquired if there was another child, and was told that there was. The average amount of liquor amnii was voided during this delivery, and a very considerable red flow followed.

No contraction taking place for some twenty or thirty minutes, and the hæmorrhage continuing with a gradual increase, I reluctantly decided to deliver the placenta. Soon after commencing its detachment, the discovery of the union of the two placentæ was made. As soon as this was ascertained, I ruptured the membranes of the second child, and delivered it by the feet as expeditiously as consistent. There was more than the usual amount of liquor amnii. A male child, of seven pounds' weight. Vigorous uterine contraction soon set in, and in a few minutes the double placenta, nineteen inches in length, was passed.

Now, owing to the large size of this placental attachment, it seems to me that Simpson's method could not have been practised; and in view of the fact that the detachment of that part of the placenta attached to the neck of the uterus, and in that portion subject to dilatation, as advocated by Barnes, succeeded so well, may we not reasonably suppose that we have a right to content ourselves without detaching the whole placenta in the great majority of cases of placenta prævia? For *very seldom* does it occur that the attachment is over the os, on all sides equally.

It seems to me that this is a case in which there can be no doubt that the twin conception, occasioning so large a placenta, was the cause, in part, of its being attached so low down.

3.—*How do the Spermatozoa enter the Uterus?* By JOSEPH R. BECK, M. D., Fort Wayne, Indiana. [St. Louis Medical and Surgical Journal, September, 1872.]

After a careful review of the various opinions on this subject, Dr. Beck presents the following observations as affording the most rational answer yet suggested to the question of the mode of ingress to the uterus of the spermatic fluid:

"*August* 7, 1872.—Mrs. H. L., aged thirty-two years—of strongly-marked nervous temperament—blonde—married eight years—has one child, son, living, seven years old—has had one abortion—last pregnancy was six years ago—commenced to menstruate at fourteen years of age—present illness has existed six years, dating evenly with abortion—symptoms, which have been apparent during its course, were dragging and weight in pelvis, more or less pain in back and loins, slight vesical and rectal irritation, inability to walk without great fatigue, inability to lift weight of any moment, slight leucorrhœa, and a sinking sensation referred to the epigastric region—supposed the cause to be a 'falling of the womb' —present condition as regards menstruation, menstruates regularly every twenty-eight days, normal as to amount, and suffers no pain of any moment—leucorrhœa, very slight as to amount, exists all the time, is white, glairy, and unmistakably uterine—pain is intermittent, by no means severe, and is referred to the back, loins, inguinal and sacral regions—locomotion is impeded to a great extent by the consequent fatigue—as to other symptoms, she is usually constipated, and has a copious eruption of acne upon the face—of physical signs, the touch shows the os uteri just inside the vulvæ, the speculum was not used, the probe shows the pelvic-uterine axis to be changed considerably, but no flexion of uterus, and probe enters cavity two and one-half inches—diagnosis, prolapse of the uterus in the second stage—treatment, mechanical support to the uterus by means of McIntosh's stem-pessary, and internally ferruginous tonics, iodide of potassium, and liq. potass. arsenitus." Thus much for the history of the case, as compiled from an office examination.

Calling at the residence of the patient next day, for the purpose of adjusting the uterine supporter, I made an examination by the touch, and, upon introducing my finger between the pubic arch and the anterior lip of the prolapsed cervix, I was requested by her to be very careful in manipulating those parts, as she was very prone, by reason of her passionate nature, to have the sexual orgasm produced by very slight contact of the finger. Indeed, she stated that this had more than once occurred to her, when making digital investigation of herself. Here, then, was an opportunity never before afforded any one to my knowledge, and one not to be lost upon any consideration. Carefully separating the vulvæ with my left hand, so that the os uteri was brought clearly into view in a strong light, I swept the right forefinger across the cervix twice or three times, when, almost immediately, the orgasm occurred, and the following is what was presented to my view:

The os and cervix uteri had been firm, hard, and generally in a normal condition, with the os closed so as not to admit the uterine probe without difficulty; but immediately the os opened to the extent of fully an inch, made five or six successive gasps, drawing the external os into the cervix each time powerfully, and at the same time becoming quite soft to the touch. All these phenomena occurred within the space of twelve seconds' time certainly, and in an instant all was as before; the os had closed, the cervix hardened, and the relation of the parts had become as before the orgasm.

Now I carefully questioned my patient as to the nature of the sensations experienced by her at the period of excitement, and she is very positive that they were the same in *quality* as they ever were during coition, even before the occurrence of the prolapse; but admits that they were not exactly the same in *quantity*, believing that during coition the orgasm had *lasted longer*, although not at all or in any respect different as to sensation. I had almost forgotten to make mention of the intense congestion of the parts during the "crisis," and introduce the statement here.

When, in connection with the statement of the patient, who is a very intelligent and appreciative lady, I add my own observation to the effect that there was no inflammation of any kind present, either in the os or cervix uteri, the vagina, bladder, or rectum, and that the parts were in an entirely normal condition except as to position, I think we had the phenomena before us which are always present during coition; and the passage of the spermatic fluid into the uterus was explained fully, satisfactorily, and beyond the shadow of a doubt.

I do not doubt that many of my readers have seen in streams of water, some time during their lives, a species of fish known as the "sucker," which has a very peculiar-shaped mouth. These fish, when at rest in the water, pass the water through their mouths and out at their gills as all other fish do, but, in so doing, they make a peculiar suction motion with their mouths, in which the mouth is inverted into itself. Precisely such a motion does the uterus make during the period of sexual excitement. This is a homely illustration, I am aware, but I know of no other in Nature which answers so well.

This, then, is the explanation of the passage of the spermatozoa to their destination: the act of coition arouses some special nervous action in the uterus, which causes it to act in the manner above described, when the "crisis" arrives. I do not believe that the operation is either purely physical, or simply mechanical, but feel sure that the great nerve-centres have more or less the control of the matter, although, perhaps, this is not now demonstrable.

It had been my intention to have discussed the bearing of the facts here presented, upon sterility, as due to a malposition of the os, and its consequent inability to take upon itself the necessary action; but find that this subject has grown to such magnitude in this paper, that policy suggests that it be postponed to a future time. The views presented in connection with these facts are of course in a crude condition as yet, and will bear discussion and further observation; but I feel sure that the facts stand for something, and that they offer to the profession *the first and only reasonable answer* to the question of "How do the spermatozoa enter the uterus?"

4.—*Perineal Lacerations.* By JOSEPH G. SWAYNE, M. D., Physician-Accoucheur to the Bristol General Hospital, etc. [British Medical Journal, July 27, 1872.]

Of all the accidents attending delivery, there is none more common than partial, and none more rare than complete, laceration of the perinæum. By partial laceration I mean a rupture extending beyond the fourchette without passing through the sphincter-ani muscle; and I do not apply the term to that simple rupture of the fourchette which almost invariably takes place in a first labor. My observations, now, will be limited to these partial lacerations; and, to show how common they are, I shall refer to my notes of the last four hundred labors which I have myself attended. In all of these the condition of the perinæum after delivery was carefully observed. The result is, that there was one case of complete laceration and thirty-one cases of partial, twenty-five of which occurred, as might naturally be expected, in primiparæ and six in multiparæ. Now, as there were one hundred and one primiparæ in the four hundred cases, it follows that, in about a quarter of these, partial laceration of the perinæum took place. Such a result may at first appear startling; but I do not think it would surprise any one who is in the habit of making frequent examinations with the speculum; for it is remarkable how often one thus discovers the evidences of rather severe perineal laceration. Yet these occurrences, although so common, are very apt to be overlooked. "Out of sight, out of mind" is the general rule in such cases; partly because the practitioner, from regard for the natural feelings of delicacy of his patient, is unwilling to expose the parts to view, and so does not see the laceration either at the time of its occurrence or afterward; and partly because these accidents cause no very urgent symptoms, and therefore the patient does not direct attention to them. But, although they give rise to no very grave or urgent symptoms, yet they occasion minor inconveniences of different kinds, which it is just as well to prevent if possible. For instance, it is very common to find women complaining of great soreness of the vulva for two or three weeks after delivery, and, on examination, a severe laceration of the perinæum is discovered, which still continues in an unhealthy, irritable condition. When a sore of this kind heals after several dressings and applications of caustic, no union of the torn edges takes place, but the perinæum still remains in an incomplete condition. The result is, that an uncomfortable feeling of want of support is left; the parts remain too open, and there is a predisposition to prolapse of the uterus and vaginal walls. Although these consequences are not very grave, yet they are of sufficient importance to merit attention and inquiry as to the best means of prevention and cure. With regard to the former, my own experience leads me to indorse all that has been brought forward by various authors, especially by Dr. Graily Hewitt, in proof of the inefficiency of support for the prevention of laceration of the perinæum. I have supported the perinæum in many cases where rupture appeared imminent, and in many others of a similar character I have left that part without support; and the result from both plans has appeared to be much the same. There is seldom any great danger of rupture, until the head is protruded so far that the accoucheur can grasp the exposed portion with his thumb and fingers. When this can be done, the best plan is to press the head forward as much as possible under the pubic arch, so as to prevent the *vis a tergo* from acting so directly downward upon the perinæum. When there is any reason to apprehend laceration, it is always as well to have the part in view, so as to be able to give this kind of assistance at the right time; and if the patient be brought close to the edge of the bed, with the thighs well flexed upon the body, this may be managed with so little exposure that she is scarcely conscious of it. Besides, if laceration does occur, it is very difficult to estimate its nature and extent by the touch only, without having recourse to the sight; and that is the reason why many cases of ruptured perinæum are overlooked, until it is too late to obtain union of the wound by the first intention.

When in attendance on primiparæ, I generally, if possible, thus see what is going on, so that I can observe the nature and degree of the rupture just at the time when it takes place, and be able to treat it without loss of time. I have thus seen that, when rupture takes place, the rent usually commences at the fourchette, and extends backward; but I have almost as frequently noticed it to begin near the centre of the perinæum, by two or three small lacerations, and extend forward. These are generally the least favorable cases for union by the first intention. Almost the only case which I met with, in which the torn edges did not unite when brought together at once, was one of this description. The perinæum was, as it were, perforated in two places just in front of the sphincter ani, and from each perforation a rent extended forward, until the two lacerations met at the fourchette, so as to enclose between them an angular piece of flesh, which subsequently partly sloughed.

With respect to the predisposing causes of laceration of the perinæum, one can readily understand why the accident should be common in primiparæ, especially in those in whom, from advancing age, that part has become unduly rigid; but the most frequent cause of all, especially in severe cases, is, according to my experience, an unusual breadth of the perinæum. When this is the natural conformation of that part, it will be better, when treating the rupture, not to make any endeavors to produce union of the anterior part of the wound. If the accoucheur does his work too well in this respect, he will very likely have the mortification at a subsequent confinement of witnessing a good deal of it undone by a fresh laceration.

I once met with a curious case in which I found an undue breadth of the perinæum in a woman whom I attended in her second confinement. Her first confinement was tolerably easy, and accompanied with very slight laceration of the perinæum. Whether, during the healing process after this, adhesion of the lower edges of the labia majora took place, I cannot say; but I found, when examining during the second labor, that the perinæum was of most unusual breadth. It for a long time resisted all the natural efforts, and became so tightly spread over the head that it appeared every moment in imminent danger of rupture, while the os externum still remained small and undilatable. On examining more carefully, I found a dimple-like depression some little distance from the anterior edge, which appeared to mark the proper situation of the fourchette. I accordingly divided the part in front of this on a director, and the head was born almost immediately without causing any rupture of the remaining portion.

Besides the causes above mentioned, the large size of the child's head and unfavorable presentations, especially those where the head is expelled with the forehead under the pubic arch, will predispose to perineal laceration. In most of the six multiparæ, in whom I noticed perineal laceration, there had been only one previous confinement, and the perinæum had then escaped laceration in consequence of the small size of the first child. In the same way an unfavorable presentation in a subsequent labor may cause rupture in a woman who has escaped the accident at several previous ones.

The treatment of partial laceration of the perinæum is, I think, most satisfactory, provided it be adopted immediately after the occurrence, or rather, I should say, immediately after the termination of the third stage of labor. The best method of treatment is to bring the torn parts together by two or three sutures of fine silver wire. I have generally found that the wound healed more kindly with these than with silk ligatures. An old-fashioned semicircular needle is the best for the purpose. The needle should enter the skin about a quarter of an inch from the edge of the wound, and the sutures should pass through the tissues at about two-thirds of their depth. The operation gives but little pain, as the parts seem to be partially benumbed, and their sensibility impaired by the long-continued pressure to which they have been subjected. The number of sutures should be in proportion to the extent of the injury. I have never used more than three; most usually two are sufficient, but in slight cases I have been content with one. As a general rule, when the perinæum is not ruptured to half its extent, no treatment will be necessary. I generally remove the ligatures about six days after delivery. Dr. Graily Hewitt, however, removes them at the end of three days. I cannot help thinking that this time is too short, and that by removing them so soon we run a risk of separating the newly-formed adhesions. I have never had cause to regret leaving them so long *in situ;* indeed, as I before remarked, cases treated in the way just mentioned terminate most satisfactorily, and I scarcely ever meet with one in which union did not take place by the first intention.

I have no time here to enter on the treatment of complete laceration of the perinæum. Unless it be attended to immediately after it occurs (which is always the best plan, just as in partial lacerations), the case will be one which will ultimately need the assistance of a surgeon rather than of an accoucheur. To show how rare such an occurrence is, I may mention that I have never met with more than one instance of it in my own practice, and it happened in the following manner: I was summoned in haste one evening to see a bad case of puerperal convulsions in consultation with another practitioner. He had previously endeavored to turn, but had not been able to complete the operation, which was attended with more than usual difficulty owing to the violence of the fits. I succeeded in accomplishing delivery during one of them. After delivery, the fits became less violent, and ultimately ceased, but the patient remained in a state of partial insensibility for some days. Soon after she had fully regained consciousness, she complained to the nurse that she had lost the power of retaining her fæces. On examination, we found that the perinæum had been completely ruptured, the rent extending through the external sphincter ani into the rectum. Owing to the urgent nature of the case, we had not noticed the rupture at the time when it occurred, and I can only account for it by supposing that, from the violence of the convulsive attack just at the moment of delivery, one of the limbs of the child must have been forced through the perinæum. The patient was afterward operated upon very successfully by my colleague, Mr. Coe.

5.—*Pregnancy at an Advanced Age.* [American Practitioner, September, 1862.]

At a meeting of the Louisville College of Physicians and Surgeons, Dr. Turner Anderson reported the case of a woman aged fifty-one, who was delivered at full term one morning, by a midwife, of a still-born child. The placenta remained. The next day at noon he saw the patient, when he found the uterus well contracted, os dilated, and a small portion of the placenta and the cord protruding. There had been no hæmorrhage. On introducing his hand he discovered the upper part of the placenta firmly adherent, while that nearest the os was detached and rolled on itself. With a little manipulation he removed the entire mass. The patient made a good recovery. She had given birth to a living child seven years before. Since that time she had menstruated irregularly. She was exceedingly thin, and her hair was white. Some months before her last confinement she had consulted Dr. Anderson about an enlargement of her abdomen, which she thought was dropsical. On examination he heard the sounds of the fœtal heart. The patient had never for a moment suspected pregnancy. Dr. Anderson wished to inquire of the Fellows whether they had met with pregnancy in persons of his patient's age or older.

Dr. Speed stated that he was called once to see a lady fifty-three years of age, whose youngest child was nine years old. She had been visited twice that day by a physician of this city, now dead, who had given paregoric for what he said was belly-ache. Dr. Speed found the patient in labor, which ended safely to both mother and child. The woman declared she had no thought of being pregnant.

6.—*Criminal Abortion in India.* By V. Richards, Civil Medical Officer, Bancoorah. [Indian Medical Gazette, November 1, 1872.]

The records of the Civil Surgeon's office for the past ten years contain only twenty cases in which this crime was either suspected or proved. This number gives no idea, however, of the prevalence of the crime. The passive (and at times active) opposition of the people to the officers deputed to investigate these cases renders the detection and punishment of offenders almost impossible; moreover, the stringency of the law against any police interference, except under certain circumstances, adds greatly to the difficulty.

As illustrative of this opposition to justice, I may cite the following case which is within my own knowledge: A fœtus of about the fourth month was found in a tank; a woman residing near this tank was proved (on examination) to have recently aborted, and, notwithstanding the fact of her pregnancy being a common topic of conversation among the inhabitants of the mohullah in which she resided, the people, on being questioned by the police, denied all knowledge of the matter, and so the ends of justice were defeated.

The prohibition of widow-marriage is, of course, at the bottom of these evils. It matters not whether a widow has the reputation of being immoral, provided there is no positive living proof in the shape of an infant. Hence, in the majority of instances, fear of excommunication, with its attendant troubles and inconvenience, and not a sense of shame, leads the unfortunate creatures to become either active or passive agents in the crime. Hindoos (especially women) have the greatest horror of the possibility of their bodies, after death, being disposed of in a manner contrary to custom.

There is reason to fear that some complicity on the part of the police exists. Without such an explanation it appears to me to be very remarkable that for three consecutive years, viz., from the 12th of May, 1864, to the 4th of November, 1867, not a single case came under the notice of the medical officer of the Zillah station, and one only from November, 1867, to June, 1870; while, during the latter six months of 1870, there were no less than three cases reported. During the latter seven months of 1870, at my request, for the first time, *the bodies of all persons said to have died from snake-bite were sent in for post-mortem examination.* Immediately on the adoption of this measure, deaths from the effects of criminal abortion began to be reported. Is it probable that this is a mere coincidence? It appears to me rather that the opinion of Dr. Chevers is here particularly significant and suggestive. Dr. Chevers remarks, "It is probable that the always credible report of 'died by snake-bite' covers many atrocities of this kind" (criminal abortion).

In my opinion, collusion on the part of the people would be, to a certain extent, checked, if magistrates, periodically, at uncertain intervals, ordered the bodies of all widows and young women dying to be sent in for *post-mortem* examination. Such a step, however, would probably not be unattended with great abuses and serious inconvenience. From the fact, however, of this crime being extremely prevalent, and being popularly considered but a venial offence, strong measures, so long as their imposition is unattended with actual injustice, or any great degree of hardship, are needed. I believe the public have no conception of the extent to which this crime really exists, although it only forms 00.3 per cent. of the offences for which criminals are incarcerated. Up to date (15th of May), during this year, three cases of abortion (in each instance the fœtus was discovered) have been reported, but the offenders remain unpunished.

The professional abortionists, nativae *dhees*, who are women of the lowest castes, are well known to the prostitutes and widows of the district. They have fees ranging from a few annas to ten rupees or more, according to the social standing of the applicant. These women not unfrequently also receive the present of a *sarree* in addition to the pecuniary remuneration.

It not unfrequently happens, however, that the men with whom the women cohabit are the guilty parties, and in these cases much injury is generally done to the vagina or uterus by mechanical means having been roughly resorted to by an ignorant hand.

The number of cases is rather too small for any useful deduction with regard to the classes among whom the crime prevails.

I believe the crime to be pretty general among all classes, but more especially among the agricultural population, owing to the greater facility that exists for illicit intercourse among its members.

7.—*Incubation of Small-pox in Utero.* By THOMAS SUTTON TOWNSEND. [Medical Times and Gazette, June 1, 1872.]

The subject of variolous contagion reaching the fœtus has always been one of much interest, particularly so when associated with the fact that the parent, although doubtless transmitting the disease, may herself be so proof against its poison as to allow it to pass through her system without apparently producing any disturbance or symptoms whatever, and yet her offspring, at birth or shortly afterward, may be found affected with a disease which it has contracted while *in utero*. The following remarkable case will, I think, illustrate this fact, and add another strong proof to those we now happily have of the immense value of vaccination:

A male infant, eighteen days old, was brought to the Stanhope Street Dispensary by its mother in December last. She gave the following history of it: At its birth it was in an apparently healthy condition, but from the first it had been very fretful and restless. When five days old it became feverish, and in a day or two an eruption appeared on its face, and afterward its hands, which the mother thought was what she was pleased to term "gum rash." To use her own words, "Ten days before her coming to me white blisters had taken the places of the red points, and these had increased to their present size, and had turned yellow four or five days ago." Having no one to do any thing for her, she had postponed getting medical advice until she could come to the dispensary. When I first saw the baby, its face, head, neck, and hands, were much swollen, and covered with a copious eruption of semi-confluent small-pox. Its eyes were completely closed, and saliva was running freely from its mouth. The contents of the vesicles were purulent, and, as nearly as one could judge from the history of the case and its appearances, it was the ninth or tenth day of the eruption, or the eleventh or twelfth of the disease—the fatal days—of a severe attack of semi-confluent variola. In a few days the vesicles began to rupture, and desiccation set in. The child did well.

Now, supposing this to be a case of small-pox—and a more typical one could not have presented itself—and taking for granted the fact, which is, I believe, an established one, that the period of incubation of non-inoculated pocks is from ten to fourteen days—at any rate, more than four days—this infant must have contracted the disease while *in utero*. The mother, aged twenty-four, a strong, healthy woman, was at her usual employment until the evening before she was confined. She had not had a symptom or ailment of any kind during the time she was carrying the child or since. Her right arm presented two well-marked vaccination-scars, and her face showed here and there a "pit," from a very slight attack of small-pox when a child. "We all had it," she said; and her mother confirmed this statement. About the time of her confinement there was a good deal of small-pox in the neighborhood; she had been about very much, and had several times passed a house when the clothes and bedding of small-pox patients were being carried out to be disinfected, and on one occasion, shortly before her confinement, she had been much shocked at hearing that a friend had lost three of her children from this disease.

In a paper read before the Medical and Chirurgical Society in 1809, Dr. Jenner gave an account of two cases of small-pox infection communicated to the *fœtus in utero* under peculiar circumstances. He says it is only under particular circumstances that any proof of the presence of small-pox can be adduced in those cases in which it passes through the frame without producing eruptions or in any perceptible degree disturbing the animal functions. Such proof, however, is afforded by the obvious infection of the fœtus before birth, communicated through the mother, she being already secure from any visible occurrence of the disorder.

He gives two cases illustrating this, one of them exactly analogous to that which I have described—a lady who, a few days previous to her confinement, had met a very disgusting object whose face was covered with small-pox. The smell and appearance of the poor creature affected her much at the time; and, though she mentioned the circumstance on her return home, she had no idea her infant could become infected, having had the small-pox herself when a child. During a few days after its birth the little one seemed quite well; but on the fifth day it became indisposed, and on the seventh small-pox appeared. The pustules were very few in number, and matured completely. Some matter taken from one of them was used for inoculation, and produced the disease correctly. The mother was not sensible of any indisposition herself from this exposure, nor had she any appearance of small-pox.

The second case is that of small-pox in the fœtus, the result of inoculation in the mother five weeks before her confinement. In this case, as in the last, small-pox was produced by inoculating from the pustules.

Dr. George Pearson has described the effects of variolous infection on pregnant women. Dr. Mortimer describes the case of a lady who, when within a fortnight or three weeks of her confinement, had held a conversation, at the distance of thirty or forty yards, with a person with small-pox in a state of maturation, and gave it to her infant.

It was a saying, I believe, of Dr. Jenner, that patients whose faces are swollen a good deal for four days, and who have pretty free salivation, nearly always do well. These two points were very noticeable in the case which has been the subject of this article.

8.—*Delay of Labor after Discharge of Liquor Amnii.* [Lancet.]

At a meeting of the London Obstetrical Society, held June 5th, Dr. Matthews Duncan read a paper on "Long Delay of Labor after Discharge of Liquor Amnii." A patient was expecting her confinement in June, 1872. On the 10th of March there occurred during the night a copious flow of liquor amnii, and slight irregular pains were felt. The liquor continued to discharge freely, but not constantly. The uterus gradually diminished in bulk, and in a fortnight's time it felt not much bigger than a large adult fœtal head. On the 25th of April regular pains came on, and the child was born alive, but survived a very short time. There appeared no doubt that pregnancy continued for forty-five days after the discharge of some of the liquor amnii; and the fœtus continued to live for several weeks in a very contracted uterus. The author considered that the hypothesis resorted to by Burns in explanation of such cases, that the torn membranes may be healed, was not only without rational grounds, but contrary to all we know. Among the conditions which may be mistaken for premature evacuation of liquor amnii, Dr. Matthews Duncan mentioned discharges of urine, watery discharges, such as are sometimes observed in virgins, and whose source may be Cowper's gland or the cervix uteri, discharges from the uterus of a fluid occupying the anatomical position of hydroperionic fluid, discharge of liquor chorii, discharge of the liquor amnii of one ovum, and discharge of the fluid in a cyst described as occurring between the chorion and the amnion. In the above case, however, that the discharge was liquor amnii was proved by the subsidence of the uterine tumor, by the diminution of its bulk, by the increase of its hardness, by the complete absence of discharge of liquor amnii at the time of labor, by the compressed state of the child, and by the almost complete rubbing off of the vernix caseosa. Here the discharge of liquor amnii in occasional gushes took place till labor came on; and long after it was evident that the uterus had been for the time as completely evacuated of this fluid as it could be. But this circumstance was easily explained by the accumulation of newly-secreted liquor amnii. Winkler's researches proved that the amniotic membrane has the power of secretion and absorption in a high degree. The author believed that firm compression of the fœtus may take place without active uterine contraction; and it is firm compression by active uterine contraction that is incompatible with continuance of pregnancy, or of fœtal life, not such mere firm compression as is seen in a case of missed miscarriage or missed labor. The explanation of the partial and repeated discharges of liquor amnii by a high position of the rent in the membranes, and some sort of valvular action, seemed to the author chimerical. The great question suggested was, Why do conditions which generally induce labor fail to do so in these rare cases? In our present state of utter ignorance as to the cause of the coming on of natural labor, it is not to be wondered at that we cannot tell the cause of its failing to come on. It is highly probable that he who discovers the cause of the coming on of natural labor will also be able to explain why in these abnormal cases labor does not come on.

Dr. Snow Beck mentioned a case of a lady who, when six months pregnant, had a copious discharge of clear watery fluid. Labor came on at the natural term, and there was no escape of liquor amnii. Also another case, in which at the fourth or fifth month there was a sudden discharge of two or three quarts of perfectly clear fluid, which continued more or less all night. This recurred every evening (except during one week) for ten weeks, when she was prematurely confined, and died from hæmorrhage two hours after removal of the placenta. Dr. Beck believed this to be an exudation from the vagina, and perhaps the great venous congestion was relieved by rest during the night and again increased by being about all day. He did not think that either set of glands in the uterus was involved in its production.

Dr. Barnes believed that hydrorrhœa, especially in early pregnancy, resulted from an hypertrophied condition of the glands of the uterine mucous membrane, and this might explain Dr. Beck's second case. He did not believe in the rupture of the membranes and the rent healing. He might offer a speculation in relation to the cause of labor. Why labor did not so readily come on under provocation before the natural term of gestation was, because the nervous centres had not yet attained that remarkable degree of irritability which characterized them at full time. There was a less ready response to excito-motory stimulus.

Dr. Rasch said he found the characteristic smell of the liquor amnii a very valuable help to diagnosis in cases of alleged discharge of the waters.

SURGERY.

1.—*Case of Acrodynia, or Neuralgia at the Point of the Finger.* Service of Prof. Pancoast. [Philadelphia Medical Times, May 1, 1872].

This was the case of a boy aged eighteen, whose finger had, some weeks before, been crushed in a cog-wheel. The wound had entirely healed, but the cicatrix was so extremely sensitive that he was unable to work—any accidental touch of it giving it intense pain, which shot up along his arm.

This, Prof. Pancoast remarked, was caused by some filaments of the highly-sensitive digital nerve having been contused or torn by the accident and become subsequently involved in the cicatrix; it may be that some of the Pacinian bodies which are so numerous on this nerve have been implicated. The affection is limited to one small point of pain. The same effect is sometimes seen in sewing-women, who have punctured the pulpy end of the finger with a needle so fine as scarcely to leave a mark, the slight wound giving such trouble as to render the hand almost useless for months. The only remedy in such cases is excision of the affected part of the nerve. There are strange tumors, called painful tubercles, sometimes met with in the sensitive cutaneous nerves of the leg and arm, in size varying from a pea to a hickory-nut. They seem to be developed in the centre of the nervous trunk, and have its filaments spread over them; and are so extremely sensitive that a rude touch will frequently give rise to the epileptic aura, followed by fits. They are of such peculiar consistence—resembling fibro-cartilage—that after removal they will rebound, when thrown against a hard substance, like a ball made from the elastic cartilage at the end of a sturgeon's nose. These also, with the nerves to which they are attached, must be excised as the only means of relief.

Ether was administered, and the cicatrix, with a piece of the nerve, removed. The patient returned a week afterward, and reported himself entirely relieved.

In connection with the subject of imprisonment and injury of nerves by the contraction of cicatrices, the lecturer referred to the case of Colonel Kennedy Blood, of Brookville, in this State. He had suffered from necrosis of the lower end of the thigh-bone, from which, after some years of suffering, the dead fragments of bone were removed through an opening in the soft parts upon the inner side of the limb, near the condyle. After the wound cicatrized, pain was felt from the region of the hip, down the leg to the foot. In the course of some months this became unendurable, and his health gave way from the constant pain and the loss of rest it occasioned, no therapeutic measures affording him any relief. "In this condition he came to me with the fixed resolution of having his thigh amputated, and not to listen to any other mode of treatment. After a careful examination, I came to the conclusion that the bone had become sound, and that all the trouble arose from the imprisonment of the trunk of the long saphenous nerve in the cicatrix, pressure upon which developed lines of pain, which could be traced along the branches of the nerve to the foot; at the same time there was, by reflex sympathy, excessive neuralgic pain felt throughout the whole region of the hip on that side. This pain in the hip had been for some months very severe, and was one of his reasons for insisting upon amputation. I came to the conclusion that if the trunks of the nerves involved in the cicatrix could be removed, not only the direct but all the reflex neuralgia would be relieved, and the limb saved.

"Of course, then, I would not amputate; so we compromised: I was to try my measures, and if they failed it would then be time enough to consider the practicability of a more serious operation.

"I opened the cicatrix freely, and fell upon the trunk of the saphenous and some other nervous branches above the site of the old wound. The branches seemed unusually numerous, in consequence of their all being double the natural size—owing, probably, to thickening of their neurilemmæ. Cutting all these trunks off above, I detached them from the cicatrix that had bound them down tightly to the bone, which, being exposed by the operation, was found to be in good condition. The wound healed kindly, and from that day to this he has been entirely without neuralgia in leg, foot, or hip; and, except a little numbness in the part from which I had cut off the nervous supply, he has a strong and useful limb. In fact, he leads an active life, and is a great still-hunter of the deer, and every year sends me one of his own killing."

2.—*New Mode of operating for the Radical Cure of Varicocele.* By H. B. Davison, M. D., San Francisco. [Pacific Medical and Surgical Journal, May, 1872.]

There is a surgical disease that many persons are suffering from, of the existence of which they are ignorant. I allude to *varicocele* of the spermatic cord.

The medical examinations of the military surgeons show that nearly one person in every ten was rejected on account of that lesion. I believe that it occurs oftener as a congenital defect, than brought on by external causes.

No matter whether it exists congenitally or otherwise, it is always a source of great discomfort and suffering to the person so afflicted, physically and mentally, and if not treated palliatively by astringent lotions or the suspensory bandage, or radically by ligating the vein, atrophy of the testicle sooner or later occurs, and a corresponding loss of sexual power.

Formerly, ligation of the veins was held to be fraught with danger, but more modern operations have shown the danger to be imaginary.

The so-called "subcutaneous" ligation of the spermatic vein for the cure of varicocele is not strictly a subcutaneous operation. The scrotum is perforated through both walls, and the ligation is generally performed on the patient standing, or in a semi-recumbent position, which will cause the varicose veins to become distended. When the ligation is completed there remains a plug, or clot of blood, shut off or excluded from the circulation, which acts as an extraneous body, and greatly increases the resulting inflammation of the testicle and its coverings.

It must be conceded that the danger arising from *phlebitis* will be greater in proportion to the length of time that the ligature is allowed to remain, which usually is from six to ten days, before the enclosed vein is separated.

The method which I have adopted, and have successfully performed in all cases operated on, may be briefly described as follows:

The patient is placed in the recumbent position, and an anæsthetic administered, or not, at the option of the patient and surgeon. The varicose veins are separated from the arteries and vas deferens; the testicle held up that the veins may be emptied of all blood, so that, when ligated, no plug of blood remains between the point of ligation and the body of the testicle.

The next step in the operation will be to pass a steel or silver needle, *curved* so that it will form a little more than a half circle, armed with a strong silk ligature, well waxed, through but *one* fold of the scrotum *under* the vein to be ligated.

Pass the needle in, and manipulate it through the walls of the scrotum, draw down far enough so as to draw in some of the ligature, then guide the point of the needle (which must not be sharp, for fear of wounding some of the vesels) *up and over* the vein, and seek to make its exit from the orifice of entrance, which will not be difficult with a little gentle manipulation.

Draw through sufficient of the ligature to pass through the two holes of the silver or ivory button. Tie down with a double loop, which may be tightened again in from twelve to twenty hours.

The patient may be given a mixture of choral hydrate and elixir of opium, and left in the recumbent position with sorbefacient application to the scrotum, which should be supported by a folded towel placed beneath.

In from ten to sixteen hours the inflammation will have been sufficient to cause a plug of plastic lymph to obliterate the ligated vein, and the ligature can be removed with safety in from two to three days.

The better way to remove the ligature is to untie the loop and cut off one end close to the scrotum, then, by gentle traction on the remaining portion of the ligature, it will come away without cutting through the vein, which is unnecessary, and only adds to the danger of phlebitis and retards recovery.

By the above mode of operating, I claim three great advantages over any other means yet known to the profession:

First, by perforating only one wall of the scrotum, less pain, less inflammation, and less risk of adhesion of the wounded sac and spermatic cord.

Second, by placing the patient in a recumbent posture when the operation is being performed, so that no blood may be enclosed in that portion of the vein cut off from the circulation, the resultant inflammation will be much less and the testicles will not swell so much, and absorption will be accomplished in much less time.

Third, by removing the ligature before it cuts through the vein the risk of *phlebitis* is lessened. and the patient is enabled to resume his ordinary duties much sooner.

Those who have been operated on have no return of the disease, and it would require a very close examination of the parts to discover that any operation had been performed. In one case the patient had been wearing a suspensory bandage for over twenty years, and the left testicle was much *atrophied.* It is now about sixteen months since the operation, and the testicle has regained its normal size, and the patient has a corresponding increase of sexual power.

Miscellaneous and Scientific Notes.

Tumefaction of Motor Nerve-Cells and Axis-Cylinders of the Nerve-Fibres in Certain Cases of Myelitis. By M. Charcot. (*Arch. de Physiol. Norm. et Path.*, 1, 1872.)—Three cases are briefly reported. In the first, a case of division of the spinal cord by gunshot-wound, death in about twenty-four hours, the cells were not specially implicated; there were groups of enlarged nerve-fibres scattered through the lateral and posterior columns. These fibres, on longitudinal section, were seen to be dilated only for a certain distance, and then returned to nearly or quite their normal size. The swelling affected the axis-cylinder, and the medullary sheath was distended to a narrow layer surrounding the former. The fibres measured 0.0099 mm. to 0.01 mm., the normal size being about 0.0033 mm.

The second case relates to a man, aged thirty-six years, previously healthy, who became suddenly paraplegic, and on the fifteenth day died. At the level of the sixth and seventh dorsal vertebræ the cord was found softened, and reduced to a pulp; above, the softening extended to the second or third dorsal, diminishing in extent and limited to the central part; below, it extended to the beginning of the lumbar enlargement. On examining the hardened cord the softened parts were disintegrated, but, beyond the softening, rounded or oval *foci* of softening were found in the white lateral and posterior columns, to a less number in the gray substance. There were many enlarged nerve-fibres around the *foci* of disintegration, reaching 0.045 mm. in diameter, similar to those in the previous case. Many motor nerve-cells seemed to be enlarged.

The third case is that of a woman, fifty-eight years old, who had at first in the morning pain in the nape of the neck, with sterno-mastoid contracted; five days later, incomplete paralysis. Death on the eighth day, from dyspnœa. A hæmorrhage in the left lateral portion of the cervical region was found, 4 mm. in antero-posterior diameter, and 3 mm. in transverse diameter, extending from the first cervical to the seventh cervical pair of nerves. It was only in part of recent date. In parts quite distant from the hæmorrhagic clot were enlarged nerve-fibres measuring 0.026 mm. to 0.018 mm. The nerve-cells of the anterior cornu in the vicinity of the hæmorrhage were enormous in size (*vraiment colossales*). The largest cells of the right cornu measured in their greatest diameter 0.0495 mm., those of the left cornu were 0.0825 mm.; even the smallest of these was 0.056 mm. in diameter; these cells were also abnormally rounded, as if distended to excess; their prolongations were not of the usual delicacy, they were thickened and tortuous. The substance which formed the body of these altered cells was strongly colored by carmine, finely granular, slightly opaline and opaque, for it was not easy to see the pigmentary mass and nucleus; this latter, as well as the nucleolus, seemed normal.

I am led to believe that the swelling of the axis-cylinders in these cases and also of the nerve-cells ought not to be considered merely as curiosities of pathological histology. It seems probable that these alterations will be found in quite a number of cases of acute or subacute myelitis, where, without doubt, they play an interesting part. Also in certain forms of sclerosis a similar change is found. If I am not deceived, this change should be referred to the swelling which certain anatomical elements frequently present, glandular epithelial cells, and the capsules of cartilages, under certain irritations. If this is so, we may be convinced, referring to the details of these cases, that the swelling of the axis-cylinders may be the first expression anatomically of inflammation of the spinal cord. It may exist alone, independently of all appreciable concomitant change of the medullary cylinder, of the reticulum, of neuroglia and of the blood-vessels.

In this relation the first observation is very instructive. It shows also with what rapidity the irritation may modify, in the cord, the structure of the nervous elements, even at points relatively very distant from the seat of the original disturbance. This last fact, the propagation of the irritation to a distance by the direct agency of the nerve-fibres, is also well shown in the second observation. Every thing leads to a belief, moreover, that irritation of the nerve-fibres, and more explicitly of their axis-cylinder, is in acute or subacute myelitis sometimes the initial, primordial fact (parenchymatous myelitis), sometimes a deuteropathic phenomenon, consecutive to inflammation of the connective reticulum (interstitial myelitis).

This morbid process may end, if not interfered with, in the destruction of the axis cylinders. There is but little doubt that the swollen nerve-cells may undergo an atrophy consecutive to this destruction of the axis-cylinders.

Anatomical Condition of the Muscles and Spinal Cord in a Case of Pseudo-hypertrophic Paralysis. J. M. Charcot. (*Arch. de Physiol. Norm. et Path.*, 1872, 2.) This was the twelfth case reported by M. Duchenne, in his memoir in the *Arch. Gén. de Méd.*, 1868. The patient died in hospital, and M. Duchenne sent the specimens to Charcot. The specimens were hardened in chromic acid, and consisted of portions of muscle from the deltoid, the psoas, the pectoral, and the sacro-lumbar; the cervical enlargement and the upper half of the dorsal region of the cord; several sections of the sciatic, median, and radial nerves; and a fragment from the muscular wall of the left ventricle.

The pectoral, the sacro-lumbar, and the psoas muscles, had not shown any increase in size; they showed only the first stage of the disease, the deltoid showed the ultimate stage. The former seemed nearly normal, the latter had a lardaceous appearance.

The psoas, which represents the first stage of the disease, showed on section that the thin layer of connective tissue, which normally scarcely separates the primitive muscular fibres, was replaced by thick septa, sometimes more than equal in diameter to the fibres. These septa are composed of recently-formed connective tissue, the fibres being mixed with embryoplastic nuclei and fusiform cells. Other muscles, as the pectoral, and sacro-lumbar, where the change seemed more advanced, showed fewer nuclei and cells, and the septa were formed almost exclusively of bundles of long, wavy fibrillæ, parallel to each other.

Fat-cells are mixed with the fibres, sometimes isolated, sometimes in groups, and sometimes they take the place of the fibres. This fatty substitution begins in the non-hypertrophied muscles in spots, and is quite general in the deltoid. Here the fat-tissue predominates, connective tissue and muscular fibres being seen only here and there widely separated, and in some places are wanting.

This fatty substitution is evidently the last stage of the morbid process. In the first stage, as in the psoas, the muscular fibres seem, upon a superficial inspection, normal, but a more careful examination shows that many of them are less in diameter; many are so atrophied as scarcely to be recognized; they preserve their striation even when most emaciated; neither the sheath of the sarcolemma nor the nuclei which it encloses show any alteration, and there is no granular fatty degeneration of the muscular fibres; occasionally the transverse striæ are wanting, and sometimes the longitudinal also—then they have a hyaline appearance; others are broken into fragments, and the nuclei of the sheath are seen; but both these are exceptional. M. Charcot thinks the simple substitution of the connective for the muscular tissue would not cause the hypertrophy, but that this is due to the adipose growth.

On examination of the cord, the sections, being colored by carmine, showed no change; the antero-lateral and posterior white columns were perfectly normal, the gray substance showed no alteration, the anterior cornua were neither atrophied nor deformed, the neuroglia was as transparent as usual, the motor nerve-cells were of normal number and shape and size. The anterior and posterior spinal roots were perfectly healthy. M. Charcot says, "Pseudo-hypertrophic paralysis ought to be considered as independent of all appreciable lesion of the cord or nerve-roots."

The nerves were entirely normal, and several of the nervous filaments in the pieces of muscle were normal; once a filament showed a remarkable hypertrophy of the axis-cylinder. The muscular structure of the portion of the heart was normal.

Pain and Cutaneous Sensibility in Neuralgia. Dr. H. Nothnagel. (*Virchow's Archiv*, vol. liv., 1, 2.) The author refers to Traube's statement that in all the cases which he has seen the hyperæsthesia or the analgesia can be found also in the intervals between the attacks of pain, whether the neuralgia is of central or peripheral origin. From the consideration of seventy cases he finds the statement of Traube correct, that there is always, besides the spontaneous pain, an alteration of the sensibility of the skin, either hyperæsthesia, more properly hyperalgesia, or anæsthesia; and these disturbances are in a constant and fixed relation to the duration of the neuralgia; in the commencement there is hyperalgesia; later there is diminution of sensitiveness. Equally strong irritation was much more strongly felt on the affected side, so that very light pricking with a needle caused pain, and rubbing with a metallic object, of the temperature of the room, caused general chills; but the more delicate sensibility of touch showed that there was no true hyperæsthesia; the same was found in regard to temperature, so that there was only hyperalgesia.

In most cases there is only a slight amount of anæsthesia, and so the examination must be very carefully made. Even light pricking with a needle may be felt, but the difference of a touch with the point or the head would not be recognized, or the impression would be duller on the affected side, and the points of the compass must be more widely separated for two points to be recognized.

The disturbance of cutaneous sensibility is often found limited to the region supplied by the affected nerve, but often also may extend to the whole of that side of the body.

On examining many cases of pain due to other cause than neuralgia, it was found that cutaneous hyperæsthesia or anæsthesia was present, but to a less degree than is usual in neuralgia. From this the author is led to conclude that pain as such, the physiological event of painful sensation, causes the changes in the cutaneous sensibility, the hyperæsthesia, or the anæsthesia. He considers that the cause of this may in a few cases be found in the terminal organs of the nerves, but that as a general rule we must seek in the central ganglion-cells for the changes which give rise to the cutaneous anæsthesia and hyperæsthesia in neuralgic and other varieties of pain.

Appointments, Honors, etc.—Dr. Brown-Séquard has resigned the chair of "Comparative and Experimental" Pathology in the Paris Faculty of Medicine, which he has temporarily filled since the death of Rayer. Dr. Thomas Dwight, Jr., of Boston, has been appointed Professor of Anatomy in the Medical School of Maine, at Brunswick, and Hampton E. Hill, M. D., of Augusta, has been elected Demonstrator. Dr. Alex. J. Stone, the founder and editor of the *Northwestern Medical and Surgical Journal*, has sold his interest in that publication to Dr. Henry C. Hand, of St. Paul, who assumed control of the *Journal* in July. The College of Professors in the University of Vienna have determined on adding a third teacher of Clinical Midwifery to the two already existing, and have appointed to the new post Dr. Gustav Braun, Professor of Midwifery in the Joseph's Academy. Sir John Lubbock, Bart., M. P., F. R. S., has been elected Vice-Chancellor of the University of London, in the room of Sir Edward Ryan, who accepted the office for a year only on the death of Mr. Grote.

Special Instruction in Venereal and Skin Diseases.—Dr. M. H. Henry, Surgeon to the New York Dispensary, will give a course of clinical instruction on venereal, genito-urinary, and skin diseases, at the Dispensary during the coming winter season.

The Climate of Kansas.—The following description of the climate of Kansas is given by Dr. T. Sinks, in the Leavenworth *Medical Herald:*

The State of Kansas lies between 37° and 40° north latitude, and between 94° 30′ and 102° west longitude. The eastern border is elevated nine hundred feet above the sea-level, and gradually ascends toward the west at the rate of about three feet per mile. The surface is that of gently-undulating prairie, interspersed with numerous streams, the margins of which are skirted by timber. The general direction of the principal streams is from west to east. There is an entire absence of lakes, swamps, and marshes, and the streams seldom overflow the alluvial bottoms along their course. The natural drainage of the State is most excellent, the descent of the beds of the stream being sufficiently great to produce rapid currents.

The measure of moisture precipitated in rain and snow for the year averages 31 inches at the eastern border of the State, and 24 inches at the western border, gradually decreasing toward the west.

Observations at Fort Leavenworth for thirty-five years give the following averages for the seasons: spring months, 7½ inches; summer, 13; autumn, 7½; and winter, 3½ inches. The precipitation of rain for the month of March shows a small increase over that for either of the winter months. The quantities are doubled in April and again doubled in May, the mean maximum occurring in June and the minimum in January. The geographical position of the State, its proximity to the plains, its exposure to the cold currents from the Rocky Mountains, and the warm ones from the Gulf of Mexico, the conflict of these with the great normal west wind of the temperate latitudes, all combine to produce confusion from which it is almost impossible to eliminate any thing that may be called general. However, the prevailing winds in the summer are from the southwest, south, and southeast; in the winter from the northwest, north, and northeast, in the order of frequency as enumerated. During the spring and autumn these points are about equally represented. Direct east or west winds, except of a fitful character, are quite infrequent. The average force of the winds is greater than that for the central States east of the Mississippi, and they are also more constant. The annual mean temperature at Leavenworth is 52°, at St. Louis 54°, at Cincinnati 53°, at Pittsburg 51°. There is also quite as close a resemblance in the monthly mean temperature at these points, showing, as before remarked, that the isothermal lines pretty closely correspond with the lines of latitude.

From the foregoing the natural inference would be that the prevailing diseases in Kansas were similar to those of the States lying east of us, and between the same parallels of latitude. In a general sense this is true, but the general drainage of the country, the porosity of the soil, the purity of the atmosphere, and the fact that the winters are quite dry in comparison, all contribute to reduce in amount and virulence all the malarial disorders, and in the winter particularly, inflammatory diseases, notable among which may be mentioned pneumonia and rheumatism. Adopting the nomenclature heretofore mentioned, the climate of Kansas would, we think, be very appropriately termed a bracing one, and without exaggeration a very healthy one for this latitude.

The foregoing is merely a brief review of the salient points of the subject under consideration, but it is perhaps as full as would be advisable under the circumstances. While the earth continues to revolve, the sun to shine, the winds to blow, the rain to descend, and the seasons to come and go in their regular succession, we must have ever present with us the instruments for our own destruction; much may be done, however, to blunt their keen edges, and to ward them from our vitals. Climate we cannot hope to change, but it is within the compass of human ability to obtain a knowledge of its varieties, of the diseases peculiar to each, and also to modify, to a certain degree, the baleful influences of some varieties, as well as to avail itself of the benign influence of others.

Uses of Chloride of Ammonia.—Dr. John Dewar, in the *British Medical Journal*, has the following remarks on this drug: "I consider it a very useful remedy; but, because of its very disagreeable taste, as well, perhaps, as on account of its being an old (and therefore apt to be forgotten) drug, it has not been used internally so much as its usefulness would seem to indicate. Its diaphoretic action is equal to its diuretic. It has been for a long time used more or less, especially on the Continent, in ascites. But it appears to have a special action on serous membranes generally; and I have found it very valuable in effusion into the pleura, especially when its cause is of a subacute or chronic character. Some time ago, I recorded a case of chronic hydrothorax where the effusion was very abundant, and occurring in a delicate woman, who ultimately died of phthisis. Under the use of the drug, rapid absorption took place. Since then, I have tried it in one or two other cases of a similar nature—the effusion resulting from subacute pleurisy. One of the cases occurred in a scrofulous boy, the whole of whose left chest was filled with fluid, which rapidly disappeared after taking the chloride in fifteen-grain doses every four hours. It is necessary to give to adults from twenty to thirty grains every three or four hours, in order to get its full benefit. Its precise *modus operandi* has not been clearly ascertained. Although primarily not a stimulant, it may act on serous membranes by stimulating their power of absorption. Its diaphoretic action may account for its efficacy in muscular rheumatism, in which affection I have used it very extensively, and have found it to give more relief than any other remedy I have tried. I have had personal experience of its good in muscular rheumatism coming on after scarlet fever. In these cases, also, it requires to be given in half-drachm doses. I have not found it of much use in lumbago or articular rheumatism. Some bitter infusion is the best thing to disguise its taste. I tried it mixed with sugar; but that, though it disguises its salt taste, makes it rather nauseating. With Dr. Thomson, I think this drug is deserving of further trial in other diseases, as well as in those above mentioned."

Signs of Madness in Dogs.—The Council of Hygiene, of Bordeaux, says the *British Medical Journal*, has issued the fallowing instructions: "A short time, sometimes two days, after the madness has seized the dog, it creates disturbance in the usual condition of the animal, which it is indispensable to know: 1. There are agitation and restlessness; the dog turns himself continually in his kennel. If he be at liberty, he goes and comes, and seems to be seeking something; then he remains motionless, as if waiting; he starts, bites the air, seems as if he would catch a fly, and dashes himself, howling and barking, against the wall. The voice of his master dissipates these hallucinations; the dog obeys, but slowly, with hesitation, as if with regret. 2. He does not try to bite; he is gentle, even affectionate, and he eats and drinks; but he gnaws his litter, the ends of the curtains, the padding of cushions, the coverlids of beds, the carpets, etc. 3. By the movement of his paws about the sides of his open mouth, one might think he was wishing to free his throat of a bone. 4. His voice has undergone such a change that it is impossible not to be struck by it. 5. The dog begins to fight with other dogs; this is decidedly a characteristic sign, if the dog be generally of a peaceful nature. The numbers 3, 4, and 5, indicate an already very advanced period of the disease, and the time is at hand when man will be exposed to the dangerous fits of the animal, if immediate measures be not taken. These measures are to chain him up as dangerous, or, better still, to destroy him."

Discoloration of Chinese Children.—We have often observed over the sacral region of new-born children, and during infancy, but never once in adult life, a peculiar bluish mark of considerable size. The Chinese are all familiar with it, and call it, *par excellence*, the "black" (*tsing*). From the general belief in metempsychosis, old women assert that it is caused by the dead person lying upon the cash which is placed in the coffin of the deceased. Money is added by the friends and others, we know, in such circumstances, with the view of giving wealth to the departed in the other world. Or this may probably have originated in the older idea of placing gold and other metals in coffins to prevent putrefaction. We have obtained no physiological explanation of this mark. It is not a "mother's mark."—*Report of the Pekin Hospital, by Dr. John Dudgeon.*

Glass Plates for Dissecting Tables.—A substance on which to lay the cadaver, that would not be liable to absorb, corrode, or stain, like wood, zinc, or marble, has long been a "desideratum" with students of practical anatomy. This, it is believed, has been obtained at the Medical College of the Pacific, where Professor Bentley has provided tables with glass tops. Each table consists of a single slab of glass, three-fourths of an inch in thickness, two feet four inches in width, by seven feet in length, laid in moulding-plaster. They possess all the properties of neatness, durability, and even elegance. As far as we know, the contrivance originated with Dr. Bentley.—*Pacific Medical Journal.*

Lady Doctors.—Mrs. Chaplin Ayrton has just passed her second professional medical examination (anatomy and physiology) in Paris, with the note of "*très satisfait.*" The medical faculty accepted that lady's certificates from Apothecaries' Hall and the Edinburgh University as equivalent to the *Bachelier-ès-Lettres* and *Bachelier-ès-Sciences*, and to their first professional examination. Miss Archer, an English lady, who presented herself for her first professional examination (chemistry, physics, natural history, and botany), also passed with "*très satisfait.*"

Medical Rank in the Navy.—The *Naval Register*, recently published, gives the following numbers and rank of the medical staff: 15 medical directors, with the relative rank of captain; 15 medical directors, with the relative rank of commander; 50 surgeons, with the relative rank of lieutentant-commander; 25 passed-assistant surgeons, with the relative rank of lieutenant; and 52 assistant surgeons, with the relative rank of master. Among the "volunteer officers" are 2 acting past-assistant surgeons, and 11 acting assistant surgeons.

Obituary.

From the *Medical Times and Gazette*, of August 24th, we take the following sketch of the life of Frederick Carpenter Skey, C. B., F. R. S., etc., who died in London, August 15th:

Having received an excellent preliminary education, the last year or two of which were spent at the University of Edinburgh, he returned to London, and became the pupil of the celebrated Abernethy, Surgeon to St. Bartholomew's Hospital, to whom he was articled by the Royal College of Surgeons on April 5, 1816, after payment of the usual premium of five hundred guineas, to board out of the house, the sum for *resident* pupils being double that amount. On the completion of his hospital studies he underwent his examination, and was admitted a member of the College on April 5, 1822; and, as showing how highly Mr. Abernethy appreciated the skill and ability of his pupil, he allowed him to see and prescribe for many of his patients before obtaining his diploma, and for several years after his admission as a member he permitted him to receive all the fees from those who consulted him. About the year 1826 Mr. Abernethy appointed him Demonstrator of Anatomy at the St. Bartholomew's Hospital, not, however, without creating considerable jealousy. On the death of Mr. Abernethy, the anatomical lectures fell into the hands of a successor, who claimed the profits of the lectures which Mr. Skey was giving, in consequence of which Mr. Skey resigned, and, associating himself with Drs. Todd, Hope, Marshall Hall, Pereira, and Mr. Kiernan (the now only survivor), established the Aldersgate Street School of Medicine, which soon became one of the largest schools in London. Here he lectured for ten years, his audience being largely increased by many of the Bartholomew's men who preferred his lectures on surgery to those of Lawrence. Early in his career he was offered the double appointment of Surgeon and Professor at King's College, on condition that he would resign them in three years; this he declined, and the negotiations fell to the ground. Mr. Skey's practice increasing rapidly, he removed to Grosvenor Street, where he had a large and aristocratic connection. Having been elected to the Honorary Fellowship of the Royal College of Surgeons in 1843, he was nominated and returned by the Fellows to a seat in the Council of that institution in 1848; in 1850 he was appointed Hunterian Orator, when he delivered a most eloquent address. Mr. Skey, who never "ate the bread of idleness," at once accepted the appointment, offered by his colleagues in 1852, of Professor of Human Anatomy and Surgery, and three times a week lectured to a large and discriminating audience in the theatre of the institution. In 1855 he was elected a member of the Court of Examiners; and never was there a more upright, honest member of that board—perhaps a little dogmatic. On the formation of a committee to inquire into the subject of the prevention of contagious diseases in the navy and army, appointed by the Government to meet at the Admiralty, and consisting of eight eminent members of both departments of the public service, Mr. Skey was appointed chairman, and Mr. Spencer Smith, Surgeon to St. Mary's Hospital, secretary. This committee held its sittings for about a year and a half, and the result was the passing of the "Contagious Diseases Act," a bill of great value, and, though strongly opposed in some quarters, still doing great service throughout the land. For his great and valuable labors on this occasion he deservedly obtained the "C. B."

Mr. Skey was a valuable contributor to the advancement of chirurgical knowledge. A paper on "Muscular Fibre," published in the "Transactions of the Royal Society," obtained for him the Fellowship of that learned institution. He was the author of works—"On the Treatment of Chronic Ulcers by Opium," "On the Venereal Disease," and "On Operative Surgery; two lectures delivered at the Royal College of Surgeons on the "Prevalent Treatment of Disease;" also two lectures in 1854 on the "Relative Merits of the Two Operations for Stone," lectures on "Hysteria"—which went through three editions—and many papers to the medical journals, and also several letters to the *Times* on athletics, especially in reference to excessive training for rowing, on alcoholic diseases, etc.

The immediate cause of death was ulceration of the intestine; and at the close, when the pain was excessive, he bore it with the greatest courage, supported by the presence of his two daughters (both married) and two sons, the third having left the previous evening to join his regiment, under the impression communicated by his sire that he was better.

Dr. Mifflin Wistar, a son of Dr. Caspar Wistar the elder, the second Professor of Anatomy in the University of Pennsylvania, and brother of the late Dr. Caspar Wistar, died in Philadelphia on the 19th ult., in the sixty-third year of his age. He had long since retired from active life.

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Original Communications.

Art. I.—*Puerperal Mania.*[1] By Fordyce Barker, M. D., Professor of Clinical Midwifery, Bellevue Hospital Medical College.

Case I.[2]—Mary ——, aged twenty-nine years, born in England, married, entered Bellevue October 5th, primipara; menstruated last time January 28th. Labor commenced 2 a. m., October 8th, first stage, ten hours; second stage, three and a half hours; third stage, twenty minutes. The child, male, weighed nine and a half pounds. Patient was very anæmic, but lost very little blood at the time of labor.

October 9th.—Pulse 84, respiration 18, temperature 99°.

October 10th.—Pulse 80, respiration 20, temperature 98.5°.

October 11th.—Pulse 84, respiration 20, temperature 98°, breasts full. Took two laxative pills, which moved freely twice without pain.

October 12th.—Pulse 88, respiration 20, temperature 98.5°. Has a large supply of milk; nurses, by her request, another child besides her own.

October 13th.—7 a. m., pulse 112, respiration 28, temperature 99°. Patient answers questions in an excited way; stares wildly, eyes very red, but face pale; says the other women in the ward kept her awake, and were talking all night about her. Lochiæ natural and without odor. Five P. M., pulse 120, respiration 30, temperature 99.5°. Signs from auscultation and percussion negative. Urinary secretion abundant; no albumen; has been examined every day. No pain or tenderness over the uterus, which is well contracted down in the pelvic cavity. Ordered morphia sulph., one-fourth grain.

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[1] From a work on "Puerperal Diseases" now in press of D. Appleton & Co.

[2] Reported by the house-physician, who neglected to sign his name to the report.

October 14th.—Patient became so violent in the night that it was necessary to remove her from the ward and to place her in a cell. She talks incessantly and incoherently, using most profane and obscene language. Refuses to nurse her child. Two P. M., seen by Dr. Barker. Pulse 120, respiration 36. Patient so violent and restless that it was impossible to get the temperature. Ordered beef-soup every three hours, and immediately after each time quinia sulph., gr. ij, tinc. ferri murias, gtts. xv. As patient had for some twenty-four hours absolutely refused to nurse her child, the breasts were very much swollen and hard; the following to be well rubbed in them: ℞. Ext. belladonna, ℥j, glycerine, ʒij. M. At eleven o'clock to have chloral hydrat. grs. xxx.

October 15th.—Patient is reported to have slept several hours, is very much less violent, but talks incoherently. Answers no questions. Pulse 108, respiration 24. On attempting to use the thermometer, she was apparently frightened, and immediately became very excited. The same treatment to be continued.

October 16th.—Slept a good deal during the night, is much more quiet in her movements, and is very silent generally, but at long intervals talks with great volubility and incoherency. Respiration 28, pulse 112, temperature not obtained. Her condition remained very much the same for the three following days, except that her movements were more strikingly lascivious. Says that she is Mary Magdalen, and calls her nurse sometimes Martha, and at other times Lazarus.

October 20th.—Very quiet, disposed to weep, answers questions. Asks to have the "nasty stuff" taken off her breasts. Pulse 108, respiration 24, temperature 99°. Removed back to the wards. Chloral hydrate reduced to grs. xx at bedtime.

October 21st.—Very quiet, taciturn, but occasionally strange. Asked, for the first time, for her child. Cried bitterly when she found the child could get no milk. Wishes to keep it at her breast the whole time. Has revealed to-day, for the first time, that her husband deserted her and left for Colorado with another woman, six weeks before she came into the hospital. From this time she steadily improved. The milk returned to the breasts, and she left the hospital to fill a situation as wet-nurse.

CASE II.[1]—Julia H., aged twenty-two years, single, born in Ireland, pregnant first time. Menstruated last in March, 1871. During latter part of pregnancy had some swelling of the feet and labia, but chemical examination of the urine negative. Was admitted to the hospital only the day before labor began. Labor began 7 A. M., November 9th. First stage fourteen hours. Position L. O. A. Second stage, two hours and five minutes. Pains were only moderately severe, but the patient was very nervous and excitable, and seemed to suffer a good deal. Was delivered of a healthy girl, weighing six pounds fourteen ounces, a few minutes after 11 P. M. Placenta came away ten minutes after delivery of the child. The uterus contracted well, and patient passed a quiet night.

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[1] Reported by John A. McCreary, A. M., M. D., House-physician to Bellevue Hospital.

November 10*th.*—A. M., respiration 24, pulse 68, temperature 100.5°.
P. M., " 27, " 64, " 100.5°.
Complains of pain and soreness in the chest; occasional pains in the pelvic region. Ordered Magendie's solution of morphia, gtts. x.

November 11*th.*—A. M., respiration 26, pulse 76, temperature 100°. Had a chill, beginning at 12 M., which lasted two hours, followed by high fever and sweating. During chill complained of pain in lower part of back and abdomen.

Seven P. M.—Respiration 32, pulse 148, temperature 104°. No sweating, no pain, except when she moves. Slight tenderness in inguinal region. Breasts swelling, no tympanitis. Ordered tincture aconite, gtts. iij, every hour until three doses have been taken. Quiniæ sulph., gr. v, every third hour.

November 12*th.*—9 A. M., respiration 32, pulse 104, temperature 105°.
12 M., " 32, " 108, " 105°.
3 P. M., " 30, " 108, " 104.7°.
9 P. M., " 30, " 132, " 104°.
No pain or tenderness in abdomen. Occasional pain in back, running down the legs.

November 13*th.*—A. M., respiration 32, pulse 112, temperature 105°.
7 P. M., " 32, " 100, " 101°.
Aconite stopped, continue quiniæ. Patient feels much better. Has a little milk in the breast for the first time this evening.

November 14*th.*—A. M., respiration 28, pulse 84, temperature 101.5°.
P. M., " 30, " 112, " 103.7°.
Has a little cough and some soreness of the chest, with a little pain in the lower part of the abdomen when she coughs. Some tympanitis. Ordered Magendie's sol. of morph., gtts. x, and turpentine stupes to abdomen.

November 15*th.*—A. M., respiration 25, pulse 84, temperature 101.3°.
P. M., " 24, " 96, " 102.5°.
No pain, very little tenderness. As bowels have not moved for two days, ordered laxative.

November 16*th.*—A. M., respiration 30, pulse 96, temperature 104.3°.
P. M., " 30, " 104, " 103.5°.
Nervous and excited, no pain, bowels moved, tongue cleaner.

November 17*th.*—A. M., respiration 30, pulse 96, temperature 102°.
P. M., " 30, " 109, " 104.5°.
Patient very excited. Has some pain in the stomach and over uterus.

Vaginal examination reveals tenderness on both sides of uterus, but no swelling or hardness. Quinine, gr. v, every third hour. Poultices to abdomen.

P. M.—Patient very wild. Has been nervous and hysterical ever since her confinement. Has been suffering great mental anxiety for fear that her misfortune would be known. Yesterday a friend visited her in the hospital, and told her that her seducer was married. Since then she has acted very strangely, at one time crying bitterly, then begging the nurse not to heed her, and then again becoming very violent, and has delusions as to her identity. Bowels open. Potass. bromidi, ʒ ss, at bedtime.

November 18*th*.—A. M., respiration 30, pulse 84, temperature 100.5°.
P. M., " 26, " 96, " 103.5°.

Patient more quiet, with less delusions, but still very excitable. Slept most of the night. No pain.

P. M.—Complains of pain and tenderness over the hypogastric region. Ordered poultice to abdomen and a suppository of ext. opium aq., gr. j.

November 19*th*.—A. M., respiration 30, pulse 96, temperature 101.5°.
P. M., " 34, " 112, " 104.5°.

Patient rational. Pain and soreness in right iliac region.

P. M.—Ordered tinct. aconiti rad., gtts. ij, every second hour.

November 20*th*.—A. M., respiration 30, pulse 72, temperature 99°. Patient feels better. Aconite stopped.

P. M.—Respiration 36, pulse 96, temperature 102.7°. Patient very nervous. Says she did not sleep last night. Pain, tenderness, and some tympanitis of the abdomen. Turpentine stupes, and chloral hydrat. grs. xxx.

From this date until the 25th the condition of the patient did not essentially change. She slept well under the influence of the chloral hydrat.

November 25*th*.—Respiration 22, pulse 88, temperature 97.8°. Patient feels well. No pains, and appetite good. She subsequently left the hospital perfectly well.

The cases you have just seen belong to a class which occurs very frequently in this hospital, or to quote from the "Obstetric Clinic" of Prof. Elliot: "In Bellevue we receive a great many cases of puerperal mania, on account of the fact that so large a proportion of our pregnant women are unmarried primiparæ, and because others of the poorest classes, who cannot be controlled at home, are sent to the hospital."

Since I have been connected with this hospital, now seventeen years, I have had one or more cases of this malady every time I have been on service, with but one exception. In the autumn of 1861, the first year of our international war, I had 5 cases of puerperal mania; in the spring of 1862, 3; in the autumn of 1863, following the great riots in this city, I had 6 cases; and during my present service (November and December, 1870) I have had 3. I estimate the ratio of puerperal mania to the whole number of cases of labor to be 1 in 80 in this hospital.

Now, I beg you to notice the wonderful contrast in frequency of this malady here as compared with the statistics of other hospitals in other parts of the world. Scanzoni states that in Wurzburg, in forty-six years, there were 5 cases of puerperal mania out of 7,438 confinements, that is, 1 in 1,487. He also states that the records of Prague, from 1835 to 1848, show that, in 23,347 cases of labor, there were 19 instances of puerperal mania, 1 in 1,228.

In the lying-in wards of St. Giles's Infirmary, one series of cases gives 1 case of puerperal mania in 1,888 of labor, and another series 1 in 950. McClintock and Hardy, 6,634 cases of labor, give 8 cases of puerperal mania, 1 in 816. Johnston and Sinclair (Dublin Lying-in Hospital) 26 cases of mania in 13,748 of labor, 1 in 528. At the Westminster General Lying-in Hospital there were 9 cases in 3,500 of labor, or 1 in 383. At Queen Charlotte's Lying-in Hospital there were 11 in 2,000, or 1 of mania in 182 of labor.

Now, let us look at the statistics of this disease from another point of view.

Marcé, who has written in some respects the most complete essay on this subject that has appeared, finds that the records of "Public Institution for the Insane" show that about eight per cent. of the insane are due to puerperal causes.

The statistics of Scanzoni, taken also from public institutions, some being the same as those of Marcé, also furnish a percentage of about seven per cent. as resulting from puerperal causes.

Dr. J. B. Tuke, whose valuable papers on the statistics of puerperal insanity, published in the *Edinburgh Medical Journal*, in 1865 and 1867, are the most suggestive of any thing that I have read on the subject, gives the following statement: "Between January 1, 1846, and December 31, 1864, there were 2,181 female cases of insanity treated in the Royal Edinburgh Asylum;" of these 155 were so-called puerperal cases, making a percentage of 7.1. You see that there is a remarkable agreement of authorities in regard to the proportion of insanity from puerperal causes compared with all other causes as shown by the statistics of public institutions.

Another point, not to be overlooked, is that, in private practice, probably one-half of the patients recover from this malady without entering a public institution. My own experience would lead me to suppose the proportion to be much greater than this.

At all events, I think it may reasonably be assumed as proved that fully seven per cent. of the insanity which occurs among women, in civilized and Christian communities that support insane hospitals, are due to causes connected with child-bearing.

Let me say here that the term puerperal mania is ordinarily used very loosely. Dr. Tuke, in the papers that I have just alluded to, remarks with truth and great force: "In works on midwifery and mental diseases, we find the several forms of insanity which occur during pregnancy, follow parturition, and supervene on lactation, all arranged under the common head of puerperal mania. This, with regard to the first and third divisions, is of course a misnomer, a contradiction in terms; and it seems rather curious that it should have been so long adhered to, more particularly as it tends to confuse and almost stultify deductions made from the few statistics of puerperal mania of which we are possessed. For instance, any comparison, drawn between any given number of labors and any given number of so-called puerperal cases, must lead to erroneous conclusions, if the insanity of pregnancy is confounded with puerperal mania, or if, as is the case, the anæmic insanity of lactation is confounded with either."

The 155 cases of Dr. Tuke are classified by him as follows:

Insanity of pregnancy	28
Puerperal insanity	73
Insanity of lactation	54

The first group, insanity of pregnancy, thus bearing a percentage of 18.06 on the total of 155; the second, puerperal insanity proper, 47.09; and the third, insanity of lactation, 34.08.

The insanity of pregnancy and the insanity of lactation are more frequently met with by the alienists and the physicians to insane hospitals than by the obstetrician proper; and, although my remarks will be chiefly confined to the subject of puerperal mania, I will say a few words in relation to each of these forms, and also another form, the delirium of labor.

Insanity of Pregnancy.—It is a matter of common observation that, in women of certain temperaments, habits, and education, pregnancy so modifies the nervous system as to produce morbid appetites, changes of temper and disposition, sometimes moral perversion, unnatural sadness, or a settled conviction of impending death.

The diseases of the female sexual organs often produce these reflex disturbances to such a degree as to cause real insanity, and, as it is important for all of you, who are to have the responsibility of the health and happiness of the families committed to your charge, to understand this, I will take the present opportunity to say a few words on this too neglected subject.[1]

. . . .

But pregnancy is a physiological process, and the instances in which the reflex disturbances from this condition result in insanity must be rare. I have seen but two such cases, and in both the evidence of hereditary predisposition was conclusive. One of them had repeated attacks of epilepsy, the first year of her menstrual life, and the other had been previously insane, but was supposed to have entirely recovered more than two years before her marriage. In both cases the insanity was permanent. I am indebted to others, and especially to Dr. Tuke, for what I have to say in regard to this form of insanity.

Esquirol found hereditary predisposition in more than one-third of the cases that came under his observation (5 in 13). Dr. Tuke's statistics show that primiparæ are by far the most liable to this malady, "a circumstance which might have been expected when we take into consideration the moral exciting causes, anxiety, and dread of the coming event, which exist to a greater degree in the inexperienced woman." The type of the disease is almost invariably melancholia. In the 28 cases of Dr. Tuke only 2 are reported as characterized by mania, and he believes that, in those rare instances where mania occurs, it will be found that the patient has previously been the subject of insanity in that form.

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[1] This portion of the lecture "On Insanity caused by the Diseases of the Female Sexual Organs" was published in the Boston *Gynæcological Journal*, May, 1872.

In no form of insanity is the suicidal tendency so well marked as in the melancholia of pregnancy.

In the earlier stages it seems very amenable to treatment. Cases are on record in which the insanity of pregnancy is said to have disappeared with labor, but this does not seem to be a common result. If the mental symptoms disappear before or at the time of confinement, there is a marked tendency to recurrence for a longer or shorter period of time. These cases seem to be particularly benefited by treatment in the special hospitals for insane, as the assurance of protection, the regularity, amusement, and employment, alone to be found in an asylum—above all the freedom from domestic anxiety and the misapplied sympathy of relatives—in a large majority of cases are productive of the best results.

The Delirium of Labor—This is sometimes excited by the force and intensity of the pains in the second stage. It has been described by Velpeau, Cazeaux, and more fully illustrated by the late Dr. Montgomery, of Dublin, and I suppose most who have been long in practice have occasionally met with such cases. Since the common use of anæsthetics in midwifery, these cases must be very rare. I have seen but one case in the past twenty-four years, and, as this was a very peculiar one, I will briefly relate it:

The patient, a lady of high culture and remarkable good sense, without the slightest hysterical tendency that I have ever been able to discover, awoke about five in the morning, near the end of her first pregnancy, shrieking, "I am drowning, I am drowning!" and jumped from her bed. The nurse, who was sleeping in the hall bedroom adjoining, with the door standing open, and the husband, who occupied the back-chamber, rushed in and found her tearing about the room in the most frantic manner, screaming incessantly, without listening to a word said to her. I was immediately summoned, and, living very near, was with her in a few moments. I had previously ordered chloroform in anticipation of her labor, but it required the united efforts of her husband, nurse, and other servants in the house, to hold her sufficiently quiet for me to bring her under the influence of the anæsthetic. I overwhelmed her with the chloroform as speedily as possible, and then, on making an examination and finding an arm protruding from the vulva, I delivered at once a living child by turning. The after-birth speedily followed, the binder was applied, and she was placed in a dry bed before she awoke. She had, undoubtedly, been aroused from a sound sleep by the rupture of the membranes, discharge of the waters, and escape of the child's arm. It is quite certain that less than an hour elapsed from the time of this occurrence until she awoke quite calm and quiet from the sleep of the chloroform, yet one can easily understand the emphatic declaration of her husband that this hour was an eternity to him. By my urgent injunctions no allusion to the incidents of her first labor has ever been made before the patient, and she has often expressed her surprise to me that her only recollection of it should be that, on awaking, she saw her mother holding a baby.

Insanity of Lactation.—I have seen but seven cases of this type, and these were all in consultation. All recovered from the insanity, but two died within a few months after I saw them, from phthisis. All of these were cases of melancholia. As I before remarked, the physicians to insane hospitals see these cases much more frequently than obstetricians. It is essentially due to anæmia of the brain. Dr. Tuke says that when mania occurs it is of an evanescent nature, violent while it lasts, but not associated with the obscenity observable in puerperal mania. Both forms, mania and melancholy, are readily curable when taken in time.

Puerperal Mania.—The insanity which first shows itself during the puerperal period is most properly called puerperal mania, for this is the type of the disease in a great majority of cases. In Dr. Tuke's table, 57 out of the 73 cases of puerperal insanity were cases of mania. It is my belief that, if the cases which occur in private practice during the first fortnight after labor, and which either recover within a couple of weeks or pass into the stage of dementia or melancholia, and form no part of hospital statistics, could all be aggregated, it would be found that fully 90 per cent. have the original type of mania.

Again, puerperal mania is generally manifested during the first two weeks after confinement, and by the end of the month the patients have recovered or the disease has passed into a different type.

Puerperal melancholia rarely, if ever, is developed until the latter half of the month, and these, being the most intractable, are the cases which are most likely to be transferred to insane hospitals. At least this is the result of my observation.

Puerperal mania is the form which obstetricians have most frequently to deal with. In some few rare cases, it is suddenly developed without any forewarning symptoms, but, in by far a larger number, there are very characteristic prodromic symptoms, sometimes continuing for a few days and in other instances only a few hours before the explosion. There is generally an unusual excitement of manner, although, in a few, a morbid melancholy air first attracts attention. A sudden aversion is displayed toward those who have been before best loved; an excessive loquacity, or an obstinate silence, weeping or laughing equally without a motive, a morbid sensibility to light, to noises, to odors, a suspicious, watchful expression of the eye, and sleeplessness, are symptoms, which, occurring in a woman who has been confined within ten or fifteen days, indicate an impending attack of puerperal mania. There are often muscular movements of the eyelids, the face, and the hands, very much resembling the appearance of one on the brink of delirium tremens. Indeed, the general symptoms are often wonderfully like those which are characteristic of the beginning of delirium tremens, and in the case of the wife of a medical friend, which I will presently relate to you, a painful suspicion existed in the mind of her husband at first that the real disease was delirium tremens.

Then there are certain symptoms which very generally characterize the moment of the attack, but are usually of short duration. The facial expression is very peculiar, and, having once been seen, will always be remembered. The features are drawn, pallid, and the cheeks and forehead are covered with little drops of perspiration, and the whole air of the expression is unsettled, indicative of fright or fury.

But, when the malady is fully developed, the patient becomes very boisterous and noisy, incoherent in her language and in her gestures. She stares wildly at imaginary objects in the air, seizes any word spoken by those near and repeats it with "damnable iteration," clutches at every thing and every one near her, throws off all covering, jumps from the bed, and even the most refined and religious women, when possessed with the demon of puerperal mania, will scream out oaths and obscenity with a volubility perfectly astounding. Erotic manifestations occur in a majority of cases. Masturbation is sometimes noticed, but I believe, as Dr. Tuke suggests, that this is more the result of a wish to allay than excite irritation. Nearly one-half of these cases manifest a suicidal tendency, but rather as a sudden impulse than as a settled determination.

While many of these symptoms are very like those of delirium tremens, the physical symptoms are in striking contrast with those of this disease. The patient is pale, cold, clammy, with a quick, small, irritable pulse; the features are pinched, at times almost collapsed-looking. There is usually great muscular weakness, with now and then a momentary spasmodic display of unusual strength.

I wish especially to urge it upon your attention that other grave diseases may exist in a latent form, coincident with the mania, the symptoms of which are masked by the mental symptoms. In this hospital, one patient has died with pelvic peritonitis, another with pneumonia, and a third with pericarditis and endocarditis, and in neither was the disease suspected until revealed by the autopsy. All recent authors agree that phrenitis connected with puerperal mania is excessively rare.

Prognosis.—This involves the three questions of the duration of the disease—the mental recovery—and the recovery of the health. Dr. Tuke says: "Puerperal mania of itself does not kill, and when you have to combat it alone, not only death is not to be dreaded, but, in the very large proportion of cases, a return to sanity may be prognosticated. It is, perhaps, *the* most curable form of insanity. This statement is made advisedly, but does not extend to those cases which are placed under asylum treatment as a *dernier ressort.*" As to duration of the disease, in some, but comparatively few cases, it entirely disappears in a few days. I have been struck with the fact that in all the cases that I have seen, where the mania has followed puerperal convulsions, the duration of the mania has been limited to three or four days, and the patient has speedily recovered, or she has died within this period. I only mention the fact, without attempting to offer any theory to explain it.

In a majority of cases, the mania gradually subsides within a period of three weeks, more frequently earlier, and is followed by a condition of partial dementia, with some delusions, especially as regards personal identity. These gradually disappear, leaving a kind of intellectual barrenness, like one waking from a dream. From this condition you may confidently hope for ultimate recovery. In some cases, the malady is prolonged two or three or more months; but, if beyond six months, the chances of recovery are very small. When death is the result, it is almost invariably due to some associated disease, as peritonitis, or cellulitis, pneumonia, and in some very rare cases phrenitis, and the fatal result usually occurs in a very few days.

Causes.—Among the predisposing causes, hereditary tendency is the most prominent, especially traceable to the female side of the family much more frequently than to the male. This was proved to exist in 22 of the 57 cases of Dr. Tuke. Esquirol 1 in 2.8; Marcé 24 of 56; Helftt, of Berlin, 51 in 131.

The next cause which I shall mention as predisposing to this malady is hysteria. In the 73 cases of Dr. Tuke (including both mania and melancholia), the labor was complicated in 23. Dr. Tuke remarks: "The various irregularities of labor doubtless operate in different ways, those where the suffering has been long continued depressing the nervous system directly, those in which large quantities of blood have been lost producing anæmia of the brain, and, in the case of the child being still-born, a moral shock acting on the mind naturally predisposed to this affection." I will add, to those causes that I have mentioned, anæmia and eclampsia. Moral causes are no doubt among the most frequent of the predisposing causes, but they are also

Exciting Causes.—It is my firm conviction that mental emotions constitute the exciting cause of puerperal mania infinitely more frequently than all other causes combined. The relative frequency of puerperal mania is just in proportion to the susceptibility to the influence of emotional causes. In Wurzburg the proportion of cases of mania to the whole number of confinements was 1 in 1,487; in Prague, 1 in 1,228. It is not strange that Scanzoni, studying the malady in this field, should regard the frequency of mania as exaggerated, at the same time that he admits that hospital records probably do not accurately represent the relative frequency in private, as it is notoriously more common in the well-to-do classes. Now, while this is undoubtedly true in Scanzoni's field of observation, the exact reverse of this statement is true with us. I have visited the lying-in hospitals of Wurzburg, Prague, Munich, and many others in Germany, and I have conversed with Scanzoni on this very subject. From him I learned that, with most patients in these hospitals, there is no sacrifice of domestic ties or social position in going to the hospital, but, on the contrary, they are every way better off than when out of the hospital. They have never before been so well cared for. For most of them, there is no stigma of disgrace in being there, and no consciousness of moral wrong or loss of position among their associates by becoming a mother without being a wife. Among the lower classes in some parts of Germany, I believe it is considered a perfectly legitimate business for young girls to become pregnant to qualify themselves for the position of wet-nurse and earn some money. There is, then, an entire absence of those moral causes of puerperal mania which exist in tremendous force in this hospital, as I will presently show you.

Then contrast the difference in frequency between the patients in the lying-in wards of St. Giles's Infirmary, where in one series there was 1 case of mania in 1,888 confinements, and the patients of Queen Charlotte's Lying-in Hospital, where there was 1 of mania in 182 of labor.

Now, mark the difference between the moral condition of the patients in this hospital and those whose statistics I have given. A large majority of patients in our lying-in-wards are of foreign birth. They have come to a new country, many of them leaving friends behind, with the hope of improving their condition, and many are disappointed in this respect. A large proportion, probably more than one-half, are unmarried. It is impossible to ascertain the truth on this point, for many represent themselves as married and deserted by their husbands, and some of these are subsequently found to be single. But this very deceit shows a moral sense on this point. Then many, who have been seduced and abandoned by their seducers, prefer to die in the hospital rather than have their disgrace known to their relatives. In addition to this, I am well convinced that our climate has a marked influence in developing the nervous susceptibilities of Europeans who come here. Then, again, there is no part of the world where the lapse from virtue in women is so severely punished by social ostracism as in New England, and she contributes her quota of poor girls who rush to a great city to hide themselves, and are at last driven to the hospital as their only resource.

Now, in view of all these facts, I think that you will agree with me that, if statistics ever prove any thing in regard to the causes of disease, they prove that moral emotions are the great exciting cause of puerperal mania.

I will mention a curious fact that has occurred in my experience. Since 1855 I have seen thirteen cases of puerperal mania in the wives of physicians, nine in this city, and four in the adjoining cities. All but one were primiparæ. It has struck me as very extraordinary, that so large a number should have occurred in one special class, and I think this is the probable explanation: Every one of these were ladies of education and more than usual quickness of intellect, and, beginning a new experience in life, and having access to their husband's books, they probably had read just enough on midwifery to fill their minds with apprehensions as to the horrors which might be in store for them, and thus developed the cerebral disturbances, just as any other moral emotions may.

Some authors have sought to show that the exciting cause of puerperal mania was to be found in the peculiar state of the sexual system which occurs after delivery. Others would make anæmia and exhaustion the principal exciting cause.

Others, again, and most prominently the late Sir James Simpson, regard puerperal mania as especially due to a toxæmic condition of the blood, and as most frequently associated with albuminuria. Sir James Simpson suggests that "mental emotion probably acts intermediately on the mind by its morbific agency on the body." He also says that "he has only seen one instance of late years attributable to such a primary depressing mental cause, and in this case the urine was highly albuminous, as it is usually found in puerperal convulsions." Many others have seemed to adopt the views of Prof. Simpson in regard to the influence of albuminuria in developing puerperal mania. Dr. Foster Jenkins, of Yonkers, published an interesting case of puerperal mania in the *American Medical Monthly*, 1857, in which Prof. Alonzo Clark and himself found albumen abundant in the urine; the patient was treated mainly for albuminuria, and recovered. My late friend Prof. Elliot was disposed to regard albuminuria as a prominent element in causing puerperal mania, but, of the five cases of puerperal mania reported in his "Obstetric Clinic," not one was associated with albuminuria.

As for myself, since the suggestions of Sir James Simpson were first published on this subject, I have been on the constant watch for albuminuria in every case of puerperal mania that I have seen, and I have found it associated with so small a proportion of the cases, that I am compelled to regard it, when present, as simply a coincidence and not a cause. To adopt Prof. Simpson's remarks relative to anæmia and exhaustion as a cause, I should say the alleged cause is very, very often present in practice without the alleged effect following. The theory at best, if applicable at all, is applicable to a very limited number of cases, and affords no more satisfactory explanation of the origin of the disease than does the more general statement that puerperal mania results from the peculiar state of the sexual system which occurs after delivery.

Treatment.—Dr. Tuke says: "To shave and apply cold to the head, administer tartar-emetic, purge, and blister, are not uncommon remedies (!) applied where mania exists. In puerperal insanity this bad treatment insures a lapse into dementia—the patient can resist the disease, but not the remedy; each dose of antimony, each cold application, each blister, puts the case further and further beyond the control of the physician."

My own experience and observation are heartily in accord with Dr. Tuke. The most recent article on puerperal mania, which has been probably more generally read by the profession now in practice in this country, is the lecture by Sir James Simpson, in the volume of "Clinical Lectures on Diseases of Women." The warm admiration for his genius, the great respect for his remarkable talents and industry, and the deep-felt sorrow for the loss which the profession and the world sustained in his comparatively early death, combine to add force to the intrinsic weight of his suggestions. But his remarks on the treatment of puerperal mania leave the strong impression on my mind that he could not have had the personal supervision of many cases, although he probably saw a great many cases in consultation. I refer more especially to his remarks on "nervous sedatives," "specifics," and "depurants," which bear the stamp of theoretical suggestions rather than practical deductions from clinical observation.

Bleeding, once so much in vogue, it is now settled, is not only useless but positively injurious in all but very exceptional cases. A vast majority of cases are undoubtedly associated with anæmia and nervous exhaustion. In one case only, I have seen venesection positively beneficial. The patient was in a sthenic condition. She had lost very little blood at the time of labor, and the symptoms of phrenitis were very marked.

Vascular sedatives are equally useless except when the mania is complicated with evident symptoms of some latent local inflammation, a complication which cannot be too sedulously watched for.

Laxatives and emetics should never be given, except when there are positive indications of their necessity.

As insomnia is one of the most striking features of puerperal mania, opiates are naturally suggested, and I have found, in the cases that I have seen in consultation, that they have generally been tried. Dr. Tuke says: "Drugs seem of no avail; opiates, more especially, do more harm than good. A large dose, given at the very first indication of insanity, is said to have the effect of cutting short the attack; this I cannot speak to, but repeat the statement previously made: that when it has fairly established itself, although large doses of opium may moderate the intensity, they tend to prolong the period of mania."

For my own part, I have never seen opium in any doses cut short the attack, although I have often known it to be tried. I think I have seen opiates prove of great service, in some few cases, where I have believed that the mania was complicated with latent pelvic peritonitis. But it is only in such cases that I have ever found them apparently useful. Mind you, I am now speaking of mania, not of melancholia.

It is obvious that the leading indication is to allay the brain-excitement. The question is, How best to accomplish this? My answer would be:

1. By restoring exhausted nerve-power.

(*a.*) By improving the nutrition of the brain. I look upon good nutrition, a plenty of such as is easily assimilated, to be one of the most important points in the treatment of this malady. Some obstinately refuse to take any thing, but, by management, tact, and perseverance, this difficulty is generally overcome after a time.

Then, in many cases, even in the early periods of mania, you will find that tonics are of great service. Those which I most frequently recommend are, the tincture of the muriate of iron, the chlorate of potash, and the sulphate of bebeerine. The latter is greatly to be preferred to quinine, from the fact that it has much less tendency to induce cerebral congestion.

(*b.*) By inducing sleep. This is nearly as important in puerperal mania as in delirium tremens, but there is this difference: In delirium tremens, when we have secured for our patient some hours of refreshing sleep, we ordinarily find that the disease is essentially overcome. But this is not the case in puerperal mania, for I have often seen patients, in whom good sleep has been secured for nights, and yet, when awake, the maniacal condition continued for some days as before.

Still there is no doubt that every hour of good sound sleep contributes something toward the patient's recovery. Now, neither opium nor the bromide of potassium will produce sleep in maniacal patients, as a general rule. I have used the latter largely for this purpose in puerperal mania. I have often found it very useful under circumstances which I shall presently allude to, but not as an hypnotic in mania.

It is in this disease that I have found the chloral-hydrate of immense value. It apparently does not interfere in the slightest degree with any of the organic functions; it is not followed by any unpleasant secondary effects, as opium often, and bromide of potassium sometimes is; and in mania I have never yet seen it fail to induce sleep.

Soon after the discovery of the anæsthetic effect of chloroform by Prof. Simpson, I suppose, in common with many others, I anticipated great benefits from its use in puerperal mania. But I think all have been disappointed in this particular. The sleep induced was of very short duration, patients seemed in no way benefited by this sleep, and generally the excitement seemed greater after its use than before. Prof. Simpson says: "I have sometimes found that a patient, after being anæsthetized by means of chloroform, has continued to sleep on, and has afterward wakened up quite well." I am quite confident that this must be a very exceptional result, for I have never seen it.

Whatever chemists may tell us, I am certain that the effects of chloroform and of chloral-hydrate differ in many essential particulars; chloroform induces a very profound sleep, but it is of short duration. If the patient is awakened, she does not fall asleep again without a renewal of its administration. The sleep from chloral-hydrate is prolonged often for hours, and, if awakened while under the influence, she at once falls asleep again. After the sleep of chloroform, there is frequently cerebral disturbance for a few moments after waking, as there is also just before the subject comes under its influence. The sleep from chloral-hydrate is neither preceded nor followed by symptoms indicative of cerebral excitement. Chloroform is of immense value in preventing and controlling convulsions, but of no service in producing sleep and allaying excitement in the maniacal. The chloral-hydrate has very little if any influence in preventing or controlling convulsions, but is by far the best agent known for inducing sleep in puerperal mania. I usually prescribe it in fifteen or twenty grain doses, well diluted, to be repeated every two hours until the effect is produced. I have given it in thirty and forty grain doses, but I have now settled on the smaller quantity as being safer and just as efficacious, if repeated until the desired influence is obtained.[1]

2. By combating all complications.

(*a.*) **Functional.**—If there is constipation, give laxatives. If the renal secretion is deficient, of course, diuretics will be useful. It is always important to watch that the bladder does not become over-distended.

(*b.*) **Cerebral Erethism.**—Maniacal excitement often produces a cerebral erethism, shown by the flushed face and red eyes, which, no doubt, was formerly often mistaken for phrenitis. It is in just these cases that the bromide of potassium is very useful. I have often seen great benefit from giving twenty to thirty grains once in six hours. But it does not often induce sleep, and so at night I suspend the bromide, and give the chloral-hydrate.

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[1] Since this lecture has passed out of my hands for publication, my attention has been called to a very important and interesting paper, published during my absence from the country, in the NEW YORK MEDICAL JOURNAL, June, 1872, by Robert Amory, M. D., Boston, Mass., entitled "Experiments on Animals, disproving the Theory that Chloral-hydrate acts on the Organism on account of its Decomposition into Chloroform, by the Alkaline Carbonates in the Blood."

The results of these experiments seem curiously to harmonize with my observations from the clinical study of the comparative action of the chloral-hydrate on the organism.

On the other side, it should be mentioned that Dr. Oscar Liebreich, to whom the world is indebted for the discovery of the immense therapeutic value of the chloral-hydrate, has recently published a third edition of his "Treatise on the Chloral-hydrate," in which he still maintains his original theory in regard to the action of this agent by its decomposition in the blood into chloroform. His reasoning, like my own, is based purely on clinical observations, but with quite the opposite results. For example, he mentions that, in a case of gout, a dose of hydrate of chloral produced excitement, but, when the patient had been treated with carbonate of soda (*c*.) **Local Inflammations.**—Let me again warn you of the danger of overlooking the existence of these complications, as they are not manifested by the usual symptoms, being masked by the mania. The treatment must be adapted to the special form and locality of the inflammation, modified by the general condition of the patient.

In this connection, I will give you the brief history of a case which, to me, was very interesting and suggestive: In November, 1869, a medical friend asked me to see his wife, who had been confined with her sixth child, just a week. I had never before seen her, and found her pale, with a hot skin, a staring expression of the eyes, and pulse of 140. Every question asked her she answered with an abrupt negative. The day before she had taken castor-oil, and seemed to suffer a good deal of pain when the medicine acted, and for the first twenty-four hours there had apparently been a good deal of difficulty and pain in passing water. She would neither permit myself nor her husband to place a hand upon the lower part of the abdomen, and of course a vaginal examination was

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for a week, the same dose acted as an hypnotic. Dr. Liebreich's theory is, that this was due to the circumstance that, at first, the formation of urate of soda deprived the blood of its normal amount of alkali, and thus prevented the transformation of the chloral into chloroform. In confirmation of this theory, he asserts that it has been noticed in typhus, where there is an excess of alkali in the blood, that small doses of chloral readily produced sleep, while larger (even moderate) quantities gave rise to symptoms of poisoning. He also states that the hydrate of chloral has been found to act beneficially in a number of cases of puerperal convulsions, and he explains this by accepting Frerich's theory, that the convulsive attacks are connected with the transformation of urea into carbonate of ammonia, and by supposing that, besides the production of chloroform, there is a formation of hydrochloric acid which neutralizes the ammonia.

In my lecture on puerperal convulsions, it will be seen that I am compelled to differ from Dr. Liebreich, both as regards the facts and the theories, as to the action and the value of this agent in puerperal convulsions.

It seems to me that the experiments of Dr. Amory have, so far as they have gone, conclusively demonstrated that the theory of Dr. Liebreich is erroneous. It is to be hoped that Dr. Amory will continue his experiments, as he intimates his purpose of doing, to determine whether chloroform is present in the urine of a person taking chloral. F. B.

not to be thought of. When I attempted to put a thermometer in the axilla, she exhibited great resentment of manner, apparently thinking it immodest. On retiring to another room, I found that she had been a model wife, and that her husband had been accustomed to lean upon her, leaving to her the management of all his affairs, except those which were purely professional.

Three weeks before her confinement she had lost by death her eldest daughter, by a former husband, and since that period she had never been seen to weep, never spoke of her, but attended to all her duties with a silent, unnatural calmness of manner. Her labor had not been long, and was in every respect normal. The mammary secretion was less than in her former confinements, and she seemed unusually weak, and for this reason her husband had given her, for the two days previous to my seeing her, a little brandy twice a day. On the day before, she began to ask very frequently for it, and this excited the alarm of her husband. He had formerly held an official position where he had seen a great deal of delirium tremens, probably a hundredfold more than I ever saw. From the fact that, before this illness, she would only, when absolutely prescribed, even taste wine, and that now she urgently demanded brandy, and from the change of her manner, her husband had adopted the theory, terribly distressing to him, that grief had driven her to secret drinking, and that she was now on the verge of delirium tremens.

I should mention, as a curious circumstance, that he had been in active practice thirty-two years, and had never seen a case of puerperal mania, although other physicians who have been equally long in practice have told me the same thing.

I expressed the strong conviction that she had severe inflammation of the pelvic organs, and that she was about to have puerperal mania. I suggested that poultices should be kept over the lower part of the abdomen, that a suppository of the aqueous extract of opium, and three grains of the butter of cocoa, should be pushed into the rectum every third hour; that she should have beef-tea, all she could be induced to take, at short intervals; and that, as a means of bribery and corruption, to induce her to permit the use of the suppositories, she should have a tablespoonful of brandy in a half-tumbler of milk after each suppository was introduced.

The next afternoon (Sunday) I was again summoned, when I found her furiously maniacal, with all the characteristic symptoms of this malady. The poultice she had·kept on about two hours, and then took it off and threw it violently at the head of her husband. She had not permitted the use of a single suppository. She had taken no beef-tea, and but one glass of milk-and-brandy. About noon she became very violent, perfectly astounding her family by her swearing and language generally. I put her under the influence of chloroform as soon as possible, and then made a careful examination. The evidences of peri-uterine inflammation were conclusive, and the whole abdomen was very much swollen and tympanitic. On coming out from the influence of the anæsthetic, she was even more violent than before. While under the effects of the chloroform the pulse was 120 and the temperature 105°.

As I looked upon the peritoneal inflammation as the dangerous feature in this case, I recommended that all our efforts should be directed to arrest this, and that we should address no treatment to the cure of the mania. I recommended that ten drops of the tincture of the veratrum viride and three drops of Magendie's solution of morphia should be given every hour until there should be some indication for suspending or diminishing one or both of these articles. I saw her again late in the evening, at seven the next morning, and again before eleven, and at three in the afternoon. Both medicines were continued without interruption and without any apparent effect.

Early in the evening I received an urgent summons to see her, as her husband believed her to be dying. I found her under the full influence of the veratrum viride. Her pulse was full, beating slowly 44 per minute. Her face was very pale, her skin cool; she was sweating most profusely, and had vomited twice. She was constantly talking in a very low tone, very rapidly, indistinctly, and incoherently. She had taken over half an ounce of the tincture of veratrum viride and more than a drachm of Magendie's solution. I have used the veratrum viride more than thirty years, but this lady took at least four times the quantity I have ever given to any other patient before she began to show any evidence of its specific effects. The symptoms which most alarmed her husband I knew to be due to the veratrum viride, and I assured him with great confidence that she was radically better. She was very thirsty, and swallowed with avidity every thing put into her mouth. I now recommended that she should have beef-tea or milk-punch at short intervals, and no medicine, unless the pulse rose above 80, when she was to have five drops of the veratrum viride, to be repeated in such doses as might be necessary to keep it below that point. She slept none that night nor the next day, but kept up her incessant chattering in a low tone. She took the veratrum viride, three times, with three drops of the morphia, during the following twenty-four hours. She also took a very sufficient quantity of beef-tea and milk-punch.

I now proposed to give her a half-drachm of the chloral-hydrate, stating that I had never yet seen or heard of its being given in such a case, as it was quite a new medicine. Her husband consented with great reluctance. In less than ten minutes she was asleep, and continued so for seven and a half hours, except that three times during this period her husband roused her sufficiently to give her some nutrition. The chloral-hydrate was repeated the next night. The mania now gave place to occasional lucid intervals, with more or less intellectual wandering, which continued for some weeks, but perceptibly and constantly decreasing until she was perfectly restored as to her mental condition. But I regret to say that she is still a feeble woman. I examined her but a short time since, and found the uterus very decidedly enlarged and immovable in the pelvic cavity, and she suffers from the symptoms which are generally associated with this condition.

3. By such moral treatment as will best secure the patient against all causes of nervous excitement, and will tend to excite in her a desire to obtain self-control.

This is most difficult to define in words, and still more difficult to secure. It implies the greatest kindness, but no demonstrations of excessive solicitude; firmness, but no appearance of governing or controlling; incessant care and watchfulness, concealed by an air of indifference; a ready tact in turning the current of thought or will, but no contradiction or impatience, and but few nurses and still fewer friends are able to exercise all these combined qualities. The physician will better teach them to the attendants, by his own manner when with the patient, than by didactic instructions. If the moral treatment can be secured in a great measure at home, and the patient begins to show signs of manifest improvement within two or three weeks of the commencement of the attack, it is better that she should remain at home.

But if she cannot have the advantage of proper moral treatment, and especially if the malady is not positively mitigated within the puerperal month, I have no doubt that the chances of recovery will be greatly increased by placing her in an asylum, where all the benefits of moral treatment are certain to be secured. This should not be delayed too long, as all physicians to these institutions are agreed in saying that the probabilities of cure are diminished just in proportion to the duration of the disease.

There is not the same objection to the removal to insane hospitals of those who suffer from puerperal mania as exists in other forms of insanity, because this removal does not suggest the same loss of family or social position. The public are ready to accept the puerperal state as the specific cause of the overthrow of the reason, which does not imply previous weakness of intellect or mental disease, and therefore they have sound reasons for anticipating a perfect recovery.

I will only add by way of caution that, in my observation, even those who are perfectly cured generally manifest some little occasional signs of moral perversion or mental eccentricity for months, and sometimes for a year or more.

I have nothing to add in regard to puerperal melancholia, because I have literally no clinical experience in this malady. I have never seen but one case in private practice. In this hospital, we frequently have cases of this form of the disease, although it is very much more rare than mania, but as it is generally developed the latter half of the puerperal month, and as it is more chronic in its type, the patients either die of some intercurrent disease, which is often the case, or they are transferred to the asylum on Blackwell's Island.

ART. II.—*A New Case of Extirpation of the Kidney, with Remarks.* By GEORGE A. PETERS, M. D., Surgeon to St. Luke's Hospital, Surgeon to New York Hospital, etc.

MR. WILLIAM S. B., residing in Minnesota; lawyer, aged thirty-six; married. Admitted to St. Luke's Hospital, April 1, 1872. Family history good. Had gonorrhœa in 1865, but with this exception has always been in perfect health. In October, 1870, having been married eighteen months, and in as good health as he had ever enjoyed, was exposed to a thirty-mile horseback-ride in the rain, over the prairie. That night he first noticed a frequent desire to micturate; being up four or five times, and passing large quantities of clear, straw-colored urine, without pain. This continued for two weeks, the quantity and quality of urine remaining the same, and being passed every hour or two, rather more frequently at night than during the day, and without pain.

During this time the desire to micturate was at times so sudden and intense that his urine escaped him before reaching a suitable place.

After two weeks the urine became high-colored, and began to have a thick, tenacious, and brick-dust deposit, adhering to the vessel on standing. Also he began to experience pain before and after micturition, extending from the neck of the bladder to the meatus. At this time he first suffered from a lancinating pain in region of right kidney, which increased on movement and extended downward into the pelvis, and from a sense of weight in right side, as of some heavy body moving when he turned in bed. Coming under the care of a physician, he had a catheter passed twice a day, as for stricture, with the effect of greatly increasing the muco-purulent deposit.

From October, 1870, to April, 1871, there was no important change in condition or symptoms, except that he noticed occasional streaks of blood in the urine, and gradually lost strength and flesh.

During the early summer of 1871 his general health improved, the urine became clearer, with less deposit, and he had less pain on micturition.

In July, 1871, after another long, jolting horseback-ride, he noticed a swelling of right testicle, which in two or three days became red, very painful, and large enough to fill the scrotum; the swelling also extended up the spermatic cord. The pain and swelling lasted without relief for six weeks. After being treated with rest, suspensory bandage, and cold applications, it finally subsided.

As the winter of 1871 and '72 approached, he began to suffer again as in the previous year. The amount of urine was less, but it was passed more frequently; the pain on micturition returned, and the muco-purulent deposit was increased. He also again noticed occasional streaks of blood. The pain in right lumbar region was very severe, lasting for a day or two, then intermitting for three or four days. He often started from his sleep with cries, and could not lie on either side. He was again treated for vesical calculus, cystitis, and stricture, by various physicians, but without relief; in the mean time losing flesh and strength, and for a month being confined to his bed. While in bed the catheter was passed every day. On one occasion, just after the catheter had been introduced, he felt a sharp pain in the region of the bladder, "as though something had given way," and immediately passed a wineglassful of clear pus, unmixed with urine. From that time (December, 1871) till the present (April, 1872), at the close of each act of micturition, he has passed about a drachm of pus, clear and yellow, unmixed with urine or blood.

On Admission.—Patient's appetite and general condition are good, so that while in hospital he attends to some outside law business. Urinates every hour, and has some pain both before and after. He passes about a wineglassful of urine each time, and after the urine there flows about a drachm of pus, forced out with a slight effort.

In twenty-four hours he passes three pints of urine, in which there are four ounces of pus. These measurements remain about the same from day to day.

There is abundant albumen in the urine, but casts have never been found. He complains of a constant sense of weight in right lumbar region, with pain, which is sometimes very severe and paroxysmal, shooting down into the pelvis;

also pain at the end of the penis. Has severe night-sweats, which are controlled by acid. No evidence of recent disease of lungs.

Physical Examination.—On introducing steel sound No. 13, the urethra was found sensitive throughout, but it entered the bladder without obstruction, and this organ gave evidence on its contact of no unusual sensibility.

There is a tumor occupying the region of the right kidney, supposed to be the kidney itself. It extends from about an inch below the free border of the ribs to the crest of the ilium, and reaches to within four inches of the median line in front, and downward to within an inch and a half of the anterior superior spine, with a transverse measurement of four inches. What appears to be the pelvis of the kidney is the seat of the principal enlargement, and at this point deep fluctuation is distinctly made out. By rectal examination the vas deferens of the same side is found enlarged and indurated.

The right testicle is enlarged, hard, and irregular; the induration involves also the epididymis and lower part of cord. There is a small fistulous track connecting with the epididymis, which discharges a little glairy fluid when pressed upon.

April 18*th.*—General condition of patient has improved since admission; his night-sweats are generally controlled, and he takes a fair amount of out-door exercise. Urine remains as described.

May 7*th.*—In presence of Dr. Van Buren and several of my colleagues at the hospital, the patient was etherized, and the exploring trocar of Dieulafoy's *aspirateur* (size No. 2) introduced into the tumor. The puncture was made at a point midway between the last rib and the crest of the ilium, and three inches from the vertebral spines. About three ounces of clear pus flowed rapidly into the exhausted receiver, when the current suddenly ceased, and on withdrawing the canula, after in vain sounding with it for any thing having the feel of a calculus, its eyes were found plugged with small masses of granular matter, which were decided to be fragments of calculus. The puncture healed without causing the least trouble.

From the symptoms, pain, purulent discharge, and previous traces of blood in the urine after exercise; from the condition of the kidney as appreciated by palpation and the additional evidences of stone which the exploration was thought to furnish; after due consultation, the diagnosis of calculus pyelitis was decided on, and the operation of nephrotomy, with possible extirpation of the kidney, depending on the amount of degeneration, was considered advisable.

May 16*th*. *Operation*, 2.30 P. M.—The patient under the influence of ether, an incision six and three-quarter inches in length was made, extending from the lower border of the twelfth rib to the crest of the ilium, parallel to and three inches from the vertebral spines. The outer border of the quadratus-lumborum muscle was thus reached, and, by cutting through the sub-lying fat, the capsule of the kidney was easily brought into view. A long and tedious search was then made in order to bring the pelvis of the organ in sight, so that it might be opened into, and the stone, if any existed, removed.

This, however, was found impossible, owing to the fact that the pelvis was not at all distended, and also to the depth from the surface at which it lay, and the number of adhesions binding it down on all sides. Several fluctuating points were discovered on the surface of the kidney, into one of which a small exploring trocar was introduced, and an inconsiderable amount of pus (about ʒj) was withdrawn, but no stone was felt. A silver probe was substituted for the trocar, but with no better success. The probe was withdrawn, and the smallest ordinary steel urethral sound introduced. This was felt to drop into a cavity two and a half inches, but still revealed no stone. The puncture was then enlarged by a transverse incision with a blunt-pointed bistoury, sufficient to admit the finger, and the whole cavity thoroughly explored, but without success. Considering the evident uselessness of the kidney for any physiological purpose, and the damage it had already suffered in the search, it was thought best to remove it entirely. The adhesions around the kidney on the upper and lower sides were torn away with difficulty. One vein was tied separately, the other vessels and the ureter were so firmly agglutinated as to make their separation impossible. They were all secured by one strong silk ligature, and the mass cut through between it and the kidney. The cut surface was very hard and dense, looking almost cartilaginous. A few muscular arterial branches required the ligature in the early steps of the operation. Five ligatures were left in the wound, the edges of which were brought together with six deep sutures. The operation lasted nearly two and a half hours. Wound dressed with sheet-lint, compress and body bandage. Patient, evidently much depressed by the anæsthetic, was immediately put to bed and ordered hot-air bath. Brandy was also administered both by mouth and rectum.

℞. Sol. sulph. morph. Magend., ♏v, was administered hypodermically.

7 P. M.—Hot-air bath discontinued. Does not rally well.

Two ounces each of brandy-and-milk were twice given in enema, at nine and eleven P. M. Also brandy-and-milk by the mouth.

Patient first spoke at seven in the evening, and was perfectly conscious at one hour past midnight.

May 17th, 1 A. M.—Patient has gradually rallied, till now the circulation is good. Brandy objected to, and changed to sherry.

3 A. M.—Passed four ounces of urine heavily loaded with pus and cheesy matter, such as the kidney was found to contain, also a little blood. This specimen was examined by Prof. A. Flint, Jr., with the following result: Pus, about one-fifth; specific gravity, 1018; reaction, acid; albumen in large quantity; urea, grs. 2.683 per fluidounce.

5 A. M.—Again became cold, and was put in hot-air bath, which produced free perspiration. Takes nourishment well. Since last evening at six o'clock, has taken milk, Oij; brandy, ℥ij; sherry wine, ℥iv.

12 M.—Passed two ounces of urine, much clearer than the last. Continues clear in mind, and without pain.

4 P. M.—Six drachms of urine, still better in quality, and containing no albumen. Mind clear, sleeps in snatches, with loud, stertorous breathing.

May 18th.—Has had milk, Oiij, and sherry, ℥iv, during night. Patient passed water on an average every hour and a half through the night, about five drachms each time.

7.30 A. M.—℞. Sol. s. morph. Magendie, ♏v, given under the skin. Wound looking well.

From six P. M., May 17th, to noon, May 18th, patient passed sixteen ounces of urine, specific gravity 1020, the last four ounces of which, passed at one act at noon, was examined by Prof. Flint, with the following result: Color, light straw, slightly turbid; odor, strongly urinous; reaction, acid; specific gravity, 1017; albumen, a trace (accountable for by the pus); sugar, none; quantity and general appearance of deposit, white and in small quantity; urea, grs. 5.779 per fluidounce.

Microscopical appearances: Crystals, none observed; anatomical elements, a few pus-corpuscles; casts, none observed; other morphological elements, none observed.

2 P. M.—Is vomiting and having frequent eructations of bile and mucus. All nourishment and stimulants stopped, except carbonic-acid water and carbonate of ammonia in small doses.

5.30 P. M.—℞. Sol. s. morph. Magendie, ♏v, hypodermically. Carotid pulse, 128. Surface very cold.

May 19*th*, 2.30 A. M.—During night has not been able to take any thing by the mouth except carbonic-acid water and carbonate of ammonia. Enemata are immediately ejected.

4 A. M.—Slowly failing.

6 A. M.—No vomiting since 4 A. M. Pulse very feeble. Sinking.

8 A. M.—Complained of pain in wound. Wound dressed. Some oozing of bloody serum. Much exhausted.

10 A. M.—Still sinking. Has had two injections of milk-and-brandy since 8 A. M., which were held in, but with no benefit. Mind clear, except occasional slight wandering. No coma.

May 19*th*, 10.15 A. M.—Patient died slowly and without a struggle, sixty-five hours after operation. Passed two and a half ounces of urine of good quality since yesterday noon.

An autopsy was carefully made about six hours after death, under the immediate direction of Dr. Francis Delafield; a copy of his report I append:

"*Autopsy May* 19*th*, 4.30 P. M.—The body was somewhat emaciated, the skin pale, the *rigor mortis* well marked. The *head* was not examined. The *lungs* were pale and contained less than the usual amount of pigment. The right lung was bound to the chest-wall by a number of old adhesions. Some of these adhesions were infiltrated with pus, but there were no other evidences of recent inflammation. The right lung was very œdematous, its bronchi were reddened, trabeculated, and contained muco-pus. The left lung presented no lesions.

"The *heart* was of medium size. Both ventricles contained partly-decolorized clots. The valves were normal, except for a slight atheromatous thickening of the anterior leaf of the mitral valve. The *liver and spleen* were of medium size and normal appearance.

"The *large intestine* was distended with gas. The peritoneal surface of the small intestines was in some places reddened and without its usual glistening appearance, but there were no evidences of recent inflammation.

"The *peritoneal cavity* was dry. The peritonæum over the situation of the right kidney, and below this and along the course of the right ureter, was thickened, but showed no traces of recent inflammation. In this same portion of the posterior abdominal wall was a mass of indurated tissue surrounding the ureter.

"The *right ureter*, for its entire length, was dilated, thickened, its mucous membrane infiltrated with cheesy matter. It terminated above in the cavity left by the removal of the kidney. In the same cavity were seen the cut ends of the renal artery and vein, with the ligatures still in place. The artery was filled with a firm decolorized thrombus, which projected into the aorta. The vein contained a softer thrombus. Both vessels were of the ordinary size. Ligature of operation found on renal artery, three inches from aortic origin; on the vein, one and a half inch from cava.

"The *left kidney* was a little larger than usual, not congested, its tissue normal, its vessels and ureter normal.

"The *bladder* was contracted and empty. It appeared to be smaller than usual. Its walls were normal, except for a number of little papillary hypertrophies, most abundant at its neck.

"The *seminal vesicle* on the right side had its wall thickened, its mucous membrane infiltrated with cheesy matter.

"The right *epididymis* had been the seat of inflammation, with perforation through the scrotum.

"The *right kidney* was five and three quarter inches long and three inches in its greatest width. Weight two and a half ounces after it was opened down the centre. Its capsule was thick and adherent. The cortical portion was somewhat thickened. It was of a uniform, firm, succulent consistence, and of grayish color, mottled with a few red patches. The pyramids were in great part destroyed. In their place we find large cavities, with ragged, cheesy walls, encroaching in some places on the cortical substance. Neither the pelvis nor the calices were dilated, but their mucous membrane was thickened. There was but little change in the tubules. Some of them were somewhat dilated, and they partly disappeared in the new growth of interstitial tissue. The Malpighian tufts appeared normal, their capsules were dilated, and the intra-capsular epithelium was increased in size and amount. The principal lesion was the formation of fibrous tissue and small round cells between the tubules. The cell-formation was the most abundant near the edges of the cavities. At the walls of the cavities all the tissues, new and old, were converted into a granular, cheesy mass.

"It is evident that we have the same process going on in the kidney, ureter, right seminal vesicle, and right epididymis, namely, a chronic inflammation leading to the formation of fibrous tissue and lymphoid cells, with a tendency to cheesy degeneration.

"This condition is described by most writers as primary infiltrated tuberculosis of the genito-urinary organs. It is difficult to say in how many of these cases there is a new growth of tubercles, but in many of them, including the present case, there is nothing in any way resembling tubercles to be found."

On reviewing the case, these points of interest suggest themselves:

1. As to the diagnosis. From first to last the history and symptoms were such as to almost force me to the conviction

that I had a case of calculous pyelitis to deal with. The patient presented himself, on reaching the city, to my friend Prof. Wm. H. Van Buren for advice, who after careful investigation arrived at this conclusion, and sent him to me for treatment in the hospital.

The use of the *aspirateur* and the results obtained by it, as narrated in the foregoing history, seemed to complete the diagnosis to a demonstration. The softest spot presenting in the tumor was tapped, though at great depth, followed by a free flow of pus, which was apparently suddenly stopped by calculous fragments plugging up the eyes of the canula. What more could be desired? The instrument, however, as the result proved, entered the only cavity of any considerable size contained in the kidney, and the flow ceased when the cavity was evacuated. On *post-mortem* examination, no other calculous mass was found. Certainly a curious coincidence.

2. As to the operation, and what it teaches.

During the entire time of the operation the patient was under the influence of sulphuric ether, administered in the usual way with a towel and paper cone. The necessity of the case required that he should lie prone upon the belly; a very unfavorable position for the inhalation of an anæsthetic, the weight of the back and shoulders interfering greatly with the proper expansion and contraction of the chest-walls. This necessitated an occasional change of position, by elevating one or the other shoulder, thus in some degree delaying the operation.

An incision extending from lower margin of last rib to crest of ilium furnishes ample room for extirpating the kidney and securing the vessels. In this case, a kidney of nearly double the normal size was removed through this cut; demonstrating that excision of a portion of the twelfth rib, as practised by Linser, will seldom be necessary. From this operation and from my experience with other incisions on the cadaver, I believe the outer edge of the quadratus-lumborum muscle to be the best guide.

In the operation of extirpation, the peritonæum runs no danger from injury, which cannot be avoided if care is used, enucleating the kidney almost entirely with the fingers; keeping close all the while to the surface of the organ. In my case the investing connective tissue, which is usually loose, was condensed and agglutinated by the products of inflammatory action, and yet no injury was done to the peritonæum.

When the organ is enucleated and completely separated, except where its vessels enter and emerge, I would strongly advise that the bight of a stout ligature be passed beneath them, and the two ends given to an assistant to hold. By dragging upon these, he aids in lifting up the kidney as it were from the bottom of the deep wound, thus bringing things into better view; and at the same time, if any damage to artery or vein is inflicted, you have a ligature in place, by tying which you can immediately control hæmorrhage.

The time consumed in the operation was nearly two and a half hours. This was to a certain extent unavoidable on account of the difficulty experienced with the anæsthetic, but more especially from the fact that I did not find, on exposing the surface of the organ, just that condition of disease which we had been reasonably led to expect, and for which the performance of nephrotomy would have sufficed. This necessitated considerable loss of time in determining the condition as we found it, and what it was best to do.

Were the operation commenced with the fixed determination to extirpate the kidney, and the manner of its performance distinctly methodized, the time might be very much shortened, I should say by at least one-half.

The amount of blood lost was and need be comparatively little.

It will be observed that the first urine was passed ten hours after the operation, and this flow was chiefly pus, and the detritus squeezed down through the right ureter in the necessary manipulation of the diseased kidney. From this time up to noon of the 18th inst., there was a marked increase in the quantity passed, and the second analysis of Prof. Flint proved it to be of good quality; there being only a trace of pus and albumen, no casts, and a good percentage of urea.

The operation was undertaken under the firm conviction that the left kidney was not diseased, and the progress of the case up to this time confirmed me in that opinion, and gave me great hope that my patient would "pull through."

This hope, however, proved delusive. From noon of the 18th, till a quarter after 10 o'clock on the morning of the 19th, he passed but two and a half ounces of urine; this, however, was of good quality, and during this time his skin was acting freely.

He died apparently of exhaustion, without any special symptom of uræmic poisoning.

Literature of the Subject.—After diligent search I have been able to find but three other recorded cases of extirpation of the kidney.

In *Deutsche Klinik*, 1870, is recorded a case in which G. Simon, of Heidelberg, removed the entire kidney. The history, as there given, condensed, is as follows:

Patient, a woman, twenty-six years of age, had been operated upon for ovarian tumor by Simon. During the operation, the ureter was accidentally cut, leaving a urinary fistula above the symphysis pubis, communicating with the left kidney. Efforts were made to establish a connection between the ureter and the bladder, also artificial closure of the ureter. Both attempts failed. After experimenting on animals as to the effect of extirpation of one kidney on the excretion of urea, etc., and being satisfied that one healthy kidney was sufficient for the needs of the animal economy, he removed the left kidney of his patient. The woman entirely recovered from the operation, and the cure of the fistula was of course complete. At the end of six weeks she was able to sit up. The ligatures did not come away, however, until after the end of six months.

In *Wurtemberg Correspondenzblatt*, No. 61, for 1870, is the record of a case in which Dr. Linser removed the left kidney after gunshot-wound. The points of interest are briefly these:

The wound occurred in December. "Blood and water" escaped through the wound at right side of lumbar vertebra behind. In January following, suppuration was fully established. Drainage-tubes were used up to March. Casts were found in the urine. In March irregular-shaped calculi began to pass through the wound. At this time there was pain over the seat of the unwounded kidney, with symptoms of high in flammation. Urine from the bladder contained pus, blood-corpuscles, and occasional casts. After a few days these acute symptoms subsided, and the operation was performed March 23, 1871. The original wound was enlarged up and down from the twelfth rib to the ilium over the left kidney. The kidney was found expanded into a sort of cyst. In consequence of its size, the operator was obliged to excise about an inch of the twelfth rib, and to enlarge the wound laterally. Pressure controlled the hæmorrhage from the intercostal artery. Operation lasted two hours on account of dangerous symptoms from the chloroform. Loss of blood, slight. Patient died ten hours after the operation.

Autopsy.—There was no wound of the peritonæum, and no peritonitis. In the kidney removed, two openings were found communicating with track of the wound. Right kidney was diseased, having several small abscesses in its substance. Serious disease in the right kidney was considered to be the cause of death. The curious physiological fact was, abundant secretion of urine from the left kidney, and the simultaneous affection of the unwounded kidney. Both ureters were pervious.

The *British Medical Journal* of Saturday, May 18, 1872, contains a brief notice of a case in which Mr. Durham, on the Tuesday preceding (May 14th), at Guy's Hospital, removed the right kidney from a woman.

My friend Dr. Charles McBurney, Jr., of this city, was present, and has kindly furnished me with a short account of the case and operation:

"The patient was a woman, forty-three years of age, in good general health, with the following history: She had for years complained of intense pain in region of right kidney, accompanied by the ordinary symptoms of renal calculus, including hæmaturia. She had never passed calculi, however, nor had a calculus been detected in the bladder. In the early part of 1870, Mr. Durham, with the consent of the attending staff of the hospital, did the following operation: He made an incision parallel to and over the outer border of the quadratus lumborum on the right side, exposed the kidney, examined it as far as was possible without removal, found nothing abnormal about it, and decided to allow the wound to close without further operation. For some time after, the patient experienced some relief from the pain of which she had complained, but it soon recurred; was very much increased by pressure over the kidney, which could be distinctly felt beneath the cicatrix of the wound, and in May, 1872, it was decided to repeat the operation and remove the kidney.

"An incision was made in the line of the previous one, the kidney turned out and removed, the artery, vein, and ureter, having been first included in one strong ligature. The kidney was found, on incision, to be perfectly healthy. The further progress of the case has never, to my knowledge, been officially reported. I was, however, informed by a gentleman connected with the hospital, that the patient died within a week from the time of the operation, and that the *post-mortem* examination failed to reveal any cause for death, or any thing abnormal about the left kidney, the ureters, or bladder."

I find in the *British Medical Journal*, May 25, 1872, the following paragraph:

"Mr. Durham's operation of extirpation of the kidney has ended fatally."

ART. III.—*Puncture of the Bladder by Dieulafoy's Aspirator; with a Description of the Instrument.* By JAMES L. LITTLE, M. D., Surgeon to St. Luke's Hospital.

THE Pneumatic Aspirator has been in use in Paris for the past three years. Its invention is claimed by Dr. Dieulafoy, and also by Dr. Potain, of Paris, both having constructed instruments called by their respective names.

Dr. Protheroe Smith, of London, also claims to be the originator, which claim he sets forth in the *Lancet* of July 11, 1870.

These instruments, however, are the same in principle, but differ somewhat in their construction.

The principle involved in this instrument, that of removing fluids from cavities by means of capillary tubes assisted by a suction-pump, so as not to leave an open wound or admit air, has been in use in this country for many years.

Dr. Bowditch, of Massachusetts, published a paper in the *American Journal of Medical Sciences* in 1852, on the operation of paracentesis thoracis, in which he advocated the puncture of the thoracic cavity with a very small exploring needle about the size of Dieulafoy's No. 3 capillary trocar, and the removal of the fluid by means of a syringe very similar in its construction to the smaller aspirator of Dieulafoy. The principle is the same. The wound thus made being so small, there is no danger of the entrance of air, and no fear of leaving a fistulous opening. He reports fifty cases in which this operation was performed, without any evil results.

Dr. T. G. Thomas, of this city, has also for many years used a small capillary trocar with a syringe attached, for the purpose of withdrawing fluids from abdominal tumors, to assist in diagnosis.

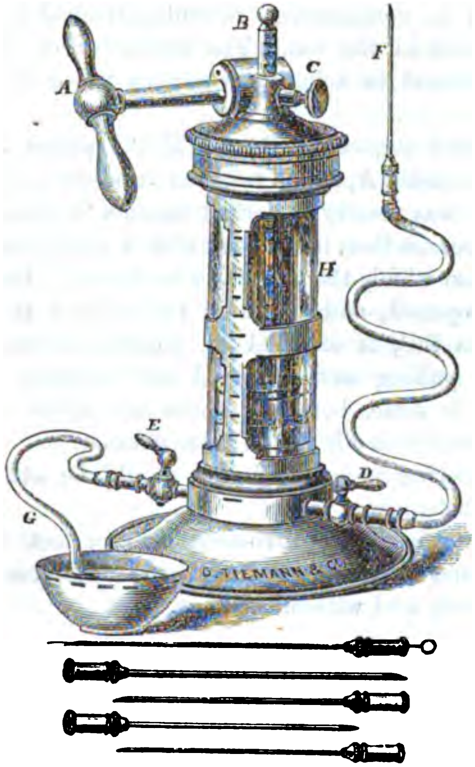
But, if the principle is not new, we certainly have to thank Dr. Dieulafoy for constructing an instrument which for convenience and safety has not been excelled, and contributing to popularize the operation, thus extending its usefulness.

The accompanying woodcut shows Dieulafoy's larger aspirator, which is now made by Messrs. Tiemann & Co., of this city.

This instrument consists of a glass cylinder, H, about 7 inches in height and 2 in diameter, with a tight-fitting piston. The cylinder is partly covered with a casing of German silver, nickel-plated. In front is a graduated scale, showing the amount of contained fluid in grammes—the gramme in this case being used as a measure of capacity and not of weight, each gramme being equal to the space occupied by a cubic centimetre of water, at the temperature of 39.2° Fahr. The cylinder holds 145 grammes, equal to nearly 4 fluidounces.

The piston B is raised or lowered by turning the handle A. Near the bottom of the cylinder are two taps with stopcocks, D and E. To these are fitted two rubber tubes, as seen in the cut.

To the extremity of the one, connected with D, a capillary trocar may be attached. About four inches from this end of the tube is inserted a piece of glass tubing about three inches in length, so as to allow the fluid to be seen passing to the cylinder. This is not shown in the woodcut.



The contents of the cylinder are discharged through the rubber tube attached to the tap E.

The capillary tubes, or trocars as they called, represented below the instrument, are six in number, sharp-pointed, and of different sizes, the smallest being about the calibre of the tube of the hypodermic syringe, or one-third of a millimetre in diameter, and the largest one and a half millimetre, and all of them four inches in length.

In addition to these tubes there should be two or three small, blunt canulas with trocars, and a detachable handle, so that, when the trocar is withdrawn, the canula may be attached to the instrument.

The manner of using the pneumatic aspirator is as follows:

The instrument is first prepared by attaching the rubber tubes to the taps D and E. After selecting the capillary trocar, it is to be connected to the tube attached to the tap D, as represented in the cut. The extremity of the tube G should be placed in a basin, to receive the contents of the cylinder.

Closing the stop-cocks D and E, the piston is raised by turning the handle A, and is retained in position by the spring C. In this way nearly a perfect vacuum is obtained. The capillary trocar is then introduced with a rotary motion into the part from which the fluid is to be drawn. The stop-cock D is then opened, and the fluid rushes into the cylinder, which, when full, is emptied by closing stop-cock D and opening E, pulling out spring C, and lowering the piston. When this is done, both stop-cocks are again closed, and another aspiration made in the same manner.

The following practical points should be attended to in using this instrument:

1. Be sure that your instrument is in perfect order—that the trocars and tubes are pervious, that the stop-cocks and piston work easily and without leakage.

2. Oil the needles or trocars before using.

3. Combine rotation with pressure in introducing the trocar, holding it between the index-finger and the thumb, and introduce slowly, so as to injure the tissues as little as possible.

Local anæsthesia may be used at the point of puncture if desired. A small nick in the integument, made with the scalpel, will facilitate the introduction of the trocar.

4. Remove the trocar slowly, and keep up the aspiration during its removal. This prevents the escape of any of the fluid which may remain in the trocar. This is always to be borne in mind whenever the peritonæum is perforated.

5. After using, carefully wash out the instrument and tubes, and insert fine wire into the sharp-pointed trocar or canula before putting it away.

6. It is well occasionally to unscrew the top of the cylinder and pour in about half an ounce of sweet-oil. This will keep the piston in good order.

This instrument has been used extensively in Paris by

Drs. Dieulafoy, Watelet, Potain, and others, for removing fluids from the pleural cavity in cases of hydrothorax or empyema; from the cranial cavity in cases of hydrocephalus; from the knee-joint in cases of synovitis; for removing pus from abscesses; for removing gas and the fluid contents of the intestines in cases of strangulated hernia, and thus allowing the gut to be reduced by taxis; also in diagnosticating hydatids and abscesses of the liver and tumors of the abdomen; and, lastly, it has been used for puncturing the bladder above the pubes to relieve retention of urine caused by stricture, enlarged prostate, or injuries of the urethra, and as yet *no case has been reported in which any trouble has followed the punctures thus made.* It has also been used to some in extent England, and in this city Drs. Sands, Weir, Markoe, Loomis, and others, have used it for various purposes, with the same results.

In this paper I wish to call the attention of the profession to its value as a means of relieving retention of urine by puncture of the bladder above the pubes. In a paper entitled "Contributions to the Surgery of the Male Urethra," published in the *New York Medical Record*, July 15, 1872, Dr. Briddon alluded to its use in Paris for retention, but, as far as I know, the following case is the first in which it has been resorted to in this country for this purpose:

CASE. *Retention from Enlarged Prostate; Fourteen Punctures.*—Mr. Raymond, aged sixty-eight; had enjoyed good health until five years ago, when he began to experience difficulty in micturition. About two years ago he had an attack of retention, which was relieved by the passage of a No. 3 silver catheter. During the night of September 5, 1872, he found himself unable to pass his urine, and in the morning sent for his physician, Dr. Samuel W. Dana, who made several ineffectual attempts to pass a catheter.

September 6th.—At 1 o'clock P. M. I was called in consultation, and found him suffering severe pain from distention of his bladder. Percussion above the pubes showed dulness as high as the umbilicus. Examination *per rectum* revealed a very great enlargement of the prostate gland. There had been considerable bleeding from the urethra, from the attempts to pass the catheter. I first endeavored to introduce a large prostatic silver catheter, well curved, but, not succeeding, tried a No. 10 gum-elastic, both with and without the stilet. This also failed to enter the bladder. The point of the catheter could be felt by the finger in the rectum near the neck of the bladder, and in spite of all my efforts it seemed to point downward and press upon the rectum instead of riding over the enlarged prostate into the bladder. Various curves of the instrument were tried, but with no better result. A vulcanized rubber, a bulbous-ended French catheter, and Squire's vertebrated catheter were also ineffectually tried. The bleeding was very considerable during these efforts. All this time the patient was in great agony from the accumulation of urine, and begging for relief.

I proposed to Dr. Dana that we should try the puncture above the pubes by means of the *Aspirateur-pneumatique sous-cutanée par Dieulafoy.* Dr. Robert F. Weir kindly loaned me his instrument, and assisted me in using it. A nick in the integument was made about an inch and a half above the pubes, and the capillary trocar No. 2, one size larger than the tube of the ordinary hypodermic syringe, was passed through the tissues into the bladder, and about 800 grammes of bloody urine removed. The pain of this operation was very slight and the relief so great that the patient fell asleep as soon as the urine began to flow, and did not awake until some time after the operation was finished. The time required to withdraw this amount of urine was about three minutes.

10 P. M.—The puncture this time was made about half an inch below the other, and about 500 grammes of urine removed. The urine was clear, and on examination was found to be of acid reaction, and, with the exception of a few blood-globules, contained nothing abnormal.

September 7th, 11 A. M.—Patient comfortable, pulse 78. Bowels moved during the night, and he passed a few drops of water at the same time.

Puncture made in a new place with a No. 1 needle, and, after 40 grammes of urine were removed, the flow ceased. This difficulty I have not been able to explain satisfactorily to myself. Another puncture was made a little higher, and nearly 500 grammes drawn out.

11 P. M.—Fifth puncture, a few lines to the right of the last, and 480 grammes of urine removed. Pulse 68; temperature normal. No tenderness at seat of punctures. Has been taking during the day tr. ferri sesquichlorid. gtt. xx, every two hours.

September 8th, 10½ A. M.—Sixth puncture just above the pubes; needle passed in with more than usual pain, and with some difficulty, without any urine making its appearance. Seventh puncture at a higher point, and 400 grammes removed. It was now deemed best to see if an entrance could be effected into the bladder by the urethra; and, after considerable difficulty, a No. 12 silver catheter was passed into the bladder. The pain was very considerable, and the bleeding quite free; and on consultation with Dr. Dana it was considered advisable to rely on the aspirator, for a day or two, to relieve his bladder and allow the urethra to become in better condition.

9 P. M.—Patient has suffered during the day from severe straining-spells, which he thinks were excited by the passage of the catheter this morning. Eighth puncture, 490 grammes.

September 9th, 10½ P. M.—Ninth puncture, 420 grammes. Free movement of bowels during the night.

7½ P. M.—Tenth puncture, 500 grammes.

11 P. M.—Eleventh puncture, about 300 grammes.

September 10th, 5½ A. M.—Twelfth puncture, 320 grammes.

Ordered balsam copaibæ ʒ ss., liq. potass. ʒ iss., aq. camphor. ℥ vj, one ounce to be taken three times a day.

2 P. M.—Thirteenth and fourteenth punctures, both of which failed to enter the bladder. In making the thirteenth puncture, the trocar, I think, passed in above the bladder, although it was not introduced higher externally than the previous punctures; this was evidently due to the position of the patient in bed, his shoulders being elevated more than usual, and consequently the bladder, which was not much distended by urine, was pressed down into the pelvis by the weight of the abdominal viscera. The fourteenth puncture failed, because the point of the instrument was too much inclined, and came in contact with the symphysis pubes. As the

patient was not suffering at the time from accumulation of urine, it was at first decided to wait a few hours before making another puncture, but afterward it was thought best to try a catheter and see if he could bear its introduction. A No. 9 English gum-elastic catheter, with the stilet and a very long curve, was carefully introduced, and passed without any pain or bleeding into the bladder, and a small quantity of urine withdrawn.

From this time his urine was removed by the catheter, in the usual way, and the remainder of the history is that of an ordinary case of enlarged prostate. No tenderness followed the punctures, and in a few days all traces of them had disappeared. The patient at last accounts was passing the greater portion of his urine without the catheter. No cystitis had taken place. During the time the aspirator was being used, he was free from all constitutional disturbance.

Remarks.—In this case the aspirator proved to be of great service. Without this instrument we would have had no other resource than puncture of the bladder through the rectum; above the pubes, in the old way; or through the perinæum—operations always more or less grave in their character. With the capillary trocar we were enabled to puncture fourteen times—three of which failed to reach the bladder, without, however, producing the slightest unpleasant result. No tenderness or pain followed any of the operations.

With regard to the failures in reaching the bladder, I think two of them may be explained by the fact that the trocar was directed at too great an inclination downward, so that the trocar came in contact with the symphysis; and in the other case the position of the patient in bed was such that the bladder, not containing much urine, was pushed down in the pelvis out of the reach of the instrument. The needle here passed in above the bladder, and of course it is impossible to know whether it penetrated the fold of the peritonæum or not.

Mr. Watelet states in a pamphlet,[1] on the puncture of the bladder by the pneumatic aspirator, that "a wound of the peritonæum or the intestine is not to be feared. He says, "un autre advantage de ce procédé, c'est que la blessure du péritoine ou de l'intestin ne sont pas à redouter. Le chirurgien n'est plus obligé de se demander s'il fait la ponction trop hard; il peut hardiment opérer." In this monograph he reports several cases in which it was used for retention of urine from stricture, from hypertrophy of the prostate, and injuries of the urethra, and in these cases not the slightest unpleasant result followed any of the punctures. He reports one case where the patient died from dysentery, during the use of the aspirator for retention from enlarged prostate; and, although the patient had received twenty-five punctures in twelve days, on *post-mortem* examination, only four very small, dark points could be discovered on the inner surface of the bladder, indicating the sites of the punctures. No abscess or infiltration of blood in the coats of the bladder could be found as the result of the punctures; and in concluding his able and valuable paper he asserts that—

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[1] "De la Ponction de la Vessie à l'aide du Trocar capillaire et de l'Aspiration pneumatique," Paris, 1871.

1. The capillary puncture is an operation entirely free from danger.
2. In all cases it should be substituted for the ordinary hypogastric puncture.
3. The bladder may be punctured three or four times a day, and replace catheterism in cases where that operation is impossible.

Now, admitting that the puncture with the capillary trocar is harmless, and, as far as we know of its use in Paris, England, and in this city, no accident has yet resulted from its use, every practical surgeon will see, at once, the invaluable aid it will furnish in this department of surgery alone.

In cases of stricture[1] where retention has come on from a night's debauch, and where we find so much difficulty and lose so much precious time in endeavoring to pass the filiform bougie so as to relieve the patient from his retention, the surgeon may with safety resort to this instrument and thus relieve the patient, and afterward take his time in finding his way through the contracted urethra; and in many cases this will be facilitated by the subsidence of the congestion of the mucous membrane by rest and antiphlogistic treatment.

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[1] *Puncture of the Bladder for Retention from Traumatic Stricture.*—On October 22d, at 9 P. M., I was called to see a patient in St. Luke's Hospital, who was suffering from retention of urine resulting from a traumatic stricture of the urethra. Finding it impossible to introduce the smallest-sized bougie, I punctured the bladder above the pubes, and withdrew the urine by means of Dieulafoy's smaller aspirator. The patient passed a comfortable night, and the following day I performed external urethrotomy.

In conclusion, I would suggest that, in using this instrument for puncture of the bladder, the following rules should be observed:

1. The patient should lie on his back, and, if the bladder is not much distended, the operation will be facilitated by slightly elevating the patient's hips by means of a pillow placed beneath them.

2. The punctures should be made on or near the median line, from one inch to one inch and a half above the pubes, and should be made each time in a different place. In the case described, the punctures were about a line apart, and extended over an area about half an inch in diameter. Mr. Watelet recommends the No. 2 capillary trocar, but, in cases where cystitis exists and the urine is loaded with pus, mucus, or the phosphates, one of the larger trocars may be used with safety.

3. The bladder may, when necessary, be washed out by filling the cylinder with water from the basin, and reversing the action of the instrument, without withdrawing the trocar from the bladder.

An interesting case of puncture of the bladder is reported by H. K. Clark, M. D., of Geneva, N. Y., in the *New York Medical Record* of June 1, 1872, occurring in the practice of Dr. Geo. N. Dox, of the same place, which shows the harmlessness of puncture above the pubes when made by a small trocar. In this case, one of retention from enlarged prostate, six or seven punctures were made with a trocar and canula one-twelfth of an inch in diameter. Each puncture was made without regard to the point of previous puncture, and the canula withdrawn as soon as the bladder was emptied. No unpleasant effects followed these operations. With the aspirator a much smaller trocar can be used; and, if perchance the peritonæum be wounded, no bad results would follow, which would not probably be the case if the injury was produced by a larger instrument.

The case of Dr. Dox is one of great interest to every practical surgeon.

Clinical Records from Private and Hospital Practice.

I.—*Report of the Surgical Cases treated in the St. John's Riverside Hospital, Yonkers, N. Y., during the Year* 1871. (*Second Year.*) By J. H. Pooley, M. D., Surgeon to the Hospital.

At the conclusion of my last year's report of the surgical cases occurring in our little hospital, I promised, should the number and interest of the cases justify it, to follow up that report by others from year to year. In fulfilment of that promise, I offer this my second annual report of the surgical practice of the St. John's Riverside Hospital, Yonkers, Westchester County, N. Y. The need of such an institution, and its growing usefulness, are sufficiently proved by the fact that more than twice as many cases have been treated in the surgical department this year as last, and more than twice as many operations performed. During the past year we have added to our staff a resident physician and surgeon, Dr. A. C. Benedict, whose faithful and conscientious services have added greatly to the comfort of the patients, and materially lightened the duties of the visiting staff.

During the year 1871 there were received into the hospital fifty-eight surgical cases, as follows, viz.:

Contusions and contused wounds	8
Fractures of the leg	8
Ulcers of the leg	6
Scalds and burns	5
Trachoma	2
Scalp-wound	2
Lacerated wound	2
Stone in the bladder	2
Orchitis	2
Fracture of the ribs	2
Concussion of the brain	2
Frost-bite	1
Chronic aural catarrh	1
Disease of knee, with amputation	1
Fracture of the arm	1
Adenitis (glands of the neck)	1
Gunshot-wound (suicidal)	1
" (accidental)	1

Fracture of cervix femoris	1
Carbuncle	1
Hip-disease	1
Sprained ankle	1
Epithelioma of face (blepharoplasty)	1
Fatty tumors (extirpation)	1
Incised wound of knee-joint	1
Fatal railroad accident	1
Synovitis (knee)	1
Retention of urine	1
Making the total number of cases	58

Of these fifty-eight cases there have been:

Discharged cured	50
" improved	8
Died in hospital	4
Remaining under treatment	1
Total	58

The number of deaths this year has been, by a singular coindence, the same as last year, and of these three have been from the result of severe injury, one having both legs crushed off above the knee by railroad accident, and only living a few hours after reaching the hospital; another, a little child, suffering from extensive scalds, dying in about two days; another from accidental gunshot-wound of the head in about five days. The fourth, whose case will be more fully noted, was admitted for fracture of the leg, subsequently operated on for stone in the bladder; died of pyelitis, caused by a large, rough calculus impacted in the kidney.

There were five operations performed during this year: one for removal of extensive epithelioma of the lower lid with blepharoplasty (which will be found reported in detail, with illustrations, in Knapp's "Archives of Ophthalmology and Otology," vol. iii., No. 1); two for stone in the bladder, both by the median operation; one amputation for joint-disease of the knee; and one for the removal of large fatty tumors, all successful.

To take up the cases we deem worthy of special notice in the order given in our list, we begin with

CONTUSIONS.

Of the eight cases of contusion and contused wounds received during the year, two were of sufficient gravity and importance to deserve record; the first being the most enormous effusion of blood from a blow which I have ever witnessed; the other a severe contusion or compression of the abdomen, producing a traumatic hernia, a true *rupture.*

CASE I. *Extraordinary Effusion of Blood from Contusion.*—John Docharty, aged fifty years, born in Ireland; admitted March 29th.

While at work on the Spuyten Duyvel & Port Morris Railroad, a large fragment of rock, which had been thrown into the air from a blast of nitro-glycerine, fell upon him, striking him upon the upper and outer part of the left thigh. Upon admission, the whole of the thigh and leg was very much swollen and discolored. The limb was very painful and tender. It was enveloped in woollen cloths wet in hot water, which were frequently removed day and night.

April 8th.—The general swelling has subsided very much, but between the knee and hip, and extending completely from the one to the other, there is a large, imperfectly-defined swelling, which fluctuates distinctly.

When struck on one side, while the hand is laid on the other, a wave of impulse is plainly perceived, similar to that in an ascitic abdomen; when grasped in the hands and shaken to and fro, the plash of semi-coagulated fluid can be clearly felt, and, as it is allowed to rest again, a tremulous motion like the vibrating of thin jelly. The fingers, thrust deeply into it, detect well-defined, rather sharp margins, like those perceived in those deceptive tumefactions of the scalp which resemble so strikingly depressions of bone.

It is evidently an enormous effusion of blood, probably under the fascia, and from the rupture of a considerable vessel.

No incision was made into it, from the fear of the extensive suppuration which must follow the exposure of such a cavity; not even a puncture for purposes of exploration was allowed.

The treatment consisted of tight bandaging and the daily inunction of mercurial ointment, under which it subsided rapidly; the bandaging gave rise to some pain, and had to be slackened occasionally.

April 11th.—Swelling has subsided a great deal; his gums are slightly sore from the blue ointment, which was accordingly discontinued.

April 17th.—His leg looks much better, the swelling is very much diminished; he has been up for the last few days. As he was very impatient to get out, he was discharged soon after, at his own request.

P. S. During the first week pulsation could not be detected in the popliteal artery, it then gradually returned. There was no pulsation in the tumor, and its surface, though discolored, showed no inflammatory blush or heat. The rapid absorption of this large sanguineous effusion struck me as very unusual, as much so as the accident itself.

CASE II. *Injury from Compression of the Body. Inguinal Hernia.*—Michael Rossi, aged forty; Italian; admitted April 7th. He was employed at Spuyten Duyvil, on the same work as the last patient, when a heavily-laden cart, backing up against him unexpectedly, squeezed him forcibly against some rocks. He had a large inguinal hernia of the oblique variety, descending into the scrotum, which had not existed before his injury, so that his intestines had been literally squeezed or forced out of his body (he was ruptured, in truth).

He had also a small swelling over the epigastrium, which felt boggy to the touch, like omentum, and disappeared slowly under firm pressure. His pulse was over 100, and he seemed to be in a great deal of pain; he was feeble, pale, cool, and apparently suffering from serious internal injury.

The hernia was reduced and a truss applied, and morphia sulph., one-quarter grain, ordered to be given every hour till the pain should be relieved.

He remained in a very curious and inexplicable condition for about a month, part of the time refusing food almost entirely, pulse kept feeble, and he constantly complained of pain over the epigastrium, increased by pressure; but, owing to the impossibility of communicating with him, as he could speak nothing but Italian, very little definite information could be obtained, and, to me at least, his case was a very mysterious one.

After being in the house nearly a month, he began gradually to improve, his bowels became regular, he began to take food, complained less and less of pain, got out of bed, and walked a little about the ward.

He was discharged, nearly well, May 18th.

The fractures of the leg, eight in number, which have been treated during the year, have been divided as follows, viz.: simple fractures—tibia alone, one; tibia and fibula, four; compound fractures—tibia alone, one; tibia and fibula, one; both bones in three places, one.

They have all been treated with the plaster-of-Paris bandage, and the results have been all that could be expected, while the pain, confinement, and inconvenience of the treatment by other means, are materially diminished and sometimes entirely abolished by this dressing.

I shall only detail three cases, the one of multiple fracture, and the other two cases of compound fracture, both of which were put up in the plaster-bandage immediately, and without any fenestrum being left over the site of the wound.

Case III.—William Hippey, aged thirty, born in England; admitted April 7th. He was engaged in quarrying out and sliding down rocks from the Palisades on the opposite side of the river, and was carried, by a mass of earth and stone that gave way under him, down to the bottom, the rocks falling upon him and rolling over and over him. The distance which he fell could not be less than two hundred feet, and it was impossible to estimate the weight which fell upon him. He had a triangular wound over the right eyebrow, extending down to the bone, which was slightly injured, a small portion of the superciliary ridge being knocked off. He had several other insignificant cuts about the head, and numerous abrasions upon the body.

His left leg was fractured in three places, one just below the knee, one about the middle of the leg, and one just above the ankle; both bones were implicated in each of these fractures; the lower one was compound, there being a wound on the outside communicating with the fibula.

The limb was greatly distorted, and, upon being handled, felt like a bag of loose bones; very free venous oozing from the compound fracture.

The wound in the forehead was closed with four silver sutures. It was found impossible perfectly to reduce the fractured leg, but, after putting it in the best possible shape, it was put up in a plaster-of-Paris bandage, a fenestrum being left at the situation of the wound over the compound fracture.

April 20th.—The bandage being loose, was removed. The wound at the site of the compound fracture was entirely healed.

No union of any of the fractures had taken place; swelling having subsided, the displacement of the fragments, which was considerable, was rectified as far as possible, and the limb again put up in a plaster-of-Paris bandage.

May 26th.—The bandage was removed, and good union of the upper and lower fractures found to have taken place, but in the middle one no union whatever; at this point the ends overlapped more than in either of the other two, it being impossible satisfactorily to reduce it at the time of the first dressing, there being no sound bone either above or below, for extension or counter-extension. It was readjusted as well as possible, and again put up in a plaster-bandage, the man being directed to take considerable daily exercise on crutches.

June 8th.—Took off the bandage; found no union—reapplied bandage. He walks about freely on crutches; general health, excellent.

July 7th.—Took off the bandage, found no union; rubbed the ends of the bones forcibly together, until crepitus could be distinctly heard and felt; then put up the limb in a fracture-box, and painted the skin over the fracture with strong tincture of iodine for three weeks, at the end of which time no change had taken place. I again put it in a plaster-bandage, which was left on for three weeks, at the expiration of which time some union, but not firm, was found to have occurred; the bandage was reapplied, soon after which, the man, becoming tired of the confinement of the hospital, went out walking on crutches. He was seen and examined four weeks after leaving the hospital, when firm union was found, and less deformity than we had expected.

This case was one of an uncommon character. It seems probable that the middle fracture failed to become consolidated for two reasons: the impossibility of attaining, much less maintaining, accurate adjustment; and the difficulty of carrying on so much osseous repair in the limb. But perseverance, after twenty-three weeks, or nearly six months, conquered, and a cure was obtained.

CASE IV.—Joseph Verico, aged thirty-three, Italian; admitted July 21st. He was injured by a bank caving in upon him. His left leg was broken at the lower third, both bones being fractured.

There was a wound, two inches long, communicating with the fracture, which bled quite freely.

The fracture was reduced, the external wound cleansed, and closed as accurately as possible with adhesive plaster, and the limb put up in the ordinary plaster-of-Paris bandage, without any fenestrum over the wound. The patient was very comfortable, complained of no pain, and was up almost daily. August 24th the bandage was taken off, and the wound found completely healed, with very little scar; but only imperfect union had taken place in the fracture. Bandage reapplied; taken off again September 10th, and good union found. Patient discharged, cured.

CASE V.—Henry Williams (colored), aged thirty-seven, born in the United States; admitted at midnight, September 28th. While walking on a railroad he was struck by a train approaching him from behind, the cow-catcher throwing him some distance.

He received a trifling scalp-wound, several bruises about the body, and a fracture of both bones of his left leg about the middle. There was a small wound on the outer side of the leg, through which the fibula protruded, and from which there was considerable venous oozing.

He was ordered an anodyne for the night, and the leg tied up in a pillow. Next morning, the wound being covered with adhesive plaster, the leg was put up in a plaster-of-Paris bandage, without any fenestrum. At the end of six weeks he had good union, the wound having closed some time previously. He made a perfect recovery.

Although we report six cases of ulcer this year, all of the leg, only one of them has been of any severity; in this, an old case of some years' standing and of considerable size, the expedient of skin-grafting was tried with complete success. As this method, as I have used it, was fully described in my last report, I shall not enter into any details of the present case, as it presents no new feature for comment. I have also had but few opportunities for the trial of skin-grafting in private practice since my last report; indeed, country practice affords but few chances for the trial of new expedients in surgery, but, in a few cases where I have tried it, it has met my expectations.

As hinted above, most of the cases of ulcer treated in the hospital this year have been really trifling affairs, which were readily and quickly healed by ordinary treatment.

There have been five cases of scalds and burns treated this year, one of them fatal, which I will give in brief:

Case VI.—Grace Howell, aged two years, born in Yonkers; admitted March 11th, 4 P. M. She had been severely scalded the same afternoon, by pulling a pot full of boiling coffee on to herself. She was most severely scalded about the neck and chest and inside of the arms; the whole of the right side of the thorax and neck up to the chin was involved, less in the left side of the chest and neck, the right arm as far as the inside of the elbow, left arm but slightly. The scalds were of the second degree of severity, leading to vesication and removal of epidermis, but no deep destruction of tissue.

She was in convulsions when brought to the hospital, which continued till eight o'clock that evening, and were succeeded by a semi-comatose condition, which lasted till midnight.

From this condition she never fairly rallied, but died the next morning. No *post mortem.*

In this case there was no opportunity for treatment; indeed, none was required.

The other four cases were all severe, three of them occurring to workmen who were engaged together in blasting rock, and accidentally exploded a large quantity of gunpowder.

The other in a man who fell down upon some heated iron in a rolling-mill.

In all these cases we found the application of carbolic acid, mixed with some oleaginous material, of the greatest efficacy in relieving pain; indeed, this application far surpasses any other that I have ever tried for this purpose: one part of pure carbolic acid to seven or eight of olive-oil will annul the severe pain of an ordinary burn almost instantly and as if by magic, another practical and useful application of this comparatively new agent.

We will next report the two cases of stone in the bladder:

CASE VII.—William Terry, aged seventeen years, born in New York; admitted April 14th, for fracture of the leg. He was a tall, pale, black-haired youth, of slender frame, and feeble constitution. While recovering from his fracture he complained of symptoms indicative of stone in the bladder, such as frequent urination, pain in passing water, etc. He said that he had been more or less troubled in this way for some years, and had had medicine prescribed for him by several physicians, but without any benefit. Upon sounding him, the presence of two moderately-sized calculi in his bladder was diagnosticated.

On June 10th the operation of median lithotomy was performed, and two smooth, oval, flattened calculi were removed, each about the size of a pigeon's-egg.

They were dark-colored externally, but lighter within, and very friable. They weighed together five hundred grains. The operation was unattended with any difficulty or circumstance worthy of note. He had complete control of his bladder from the first, and soon began to pass part of his urine *per urethram;* but, although the wound nearly healed up, it did not do so entirely, and he continued to pass part of his urine through the opening; this seemed to be owing to the state of his general health, which was very poor, but as it continued open, without any prospect of healing, it was determined to make a thorough examination. Accordingly, September 21st, he was etherized and the urethra and wound explored. There was no stricture, a No. 12 elastic catheter passed easily. On examining the wound, a fistulous opening was found communicating with the rectum, directly in the median line opposite the *raphé* of the perinæum.

By the advice of those in consultation, though somewhat contrary to my own judgment, the wound and rectum were laid freely open by an incision through the fistulous orifice, dividing the sphincter. The wound thus made gaped widely. A small opening existed in the urethra in its membranous portion. A catheter was left in the bladder for twenty-four hours, and the wound dressed with lint, as in an ordinary operation for *fistula in ano*, which it resembled.

These proceedings, however, were of no benefit to the patient; the edges of the wound continued wide apart, showed no tendency to heal, but the edges became skinned over; the urine still passed partially by the wound, and was heavily loaded with pus and mucus, and he partially lost control of his bladder, so that it soaked the sheets on which he lay; each act of urinating gave him severe pain.

October 15th.—Patient is pale, emaciated, and weak; has but little appetite, takes six ounces of whiskey a day in the form of milk-punch; has pain over the region of the bladder and kidneys. For the past few days his bladder has been washed out with warm water once a day; complains of great pain after the operation.

October 20th.—He complains of great pain in the left side, extending up into the thorax, for which hot applications were ordered. He has also a diarrhœa, for which bismuth nitr. and morphia were administered.

November 1st.—Patient is becoming weaker, has a great deal of pain in the left lumbar region, and in the bladder; washing out the bladder has been continued nearly every day for the last four weeks. Pulse 90, respiration 28, temperature 100.

November 8th.—Patient continues to grow weaker, and, as it causes him pain, and does no good, the syringing of the bladder is discontinued.

November 17th.—Patient much the same; complains constantly of pain in the kidneys and bladder, for which hot fomentations are applied and morphine administered. The whiskey disagreeing with him, he was ordered sherry wine instead. Pulse 128, respiration 32, temperature 100½. Urine is loaded with pus and mucus, and is very offensive; it all passes through the opening in the perinæum, and he has very little control over either his bladder or rectum. He still sits up a little each day.

December 2d.—Patient is becoming so weak that he can hardly sit up at all; condition in other respects unchanged since last report.

December 18th.—Patient much the same. For a week past has had night-sweats; he is obliged to take large and frequent doses of morphine to subdue pain.

December 28th.—Pain more severe. He was quieted this evening with an hypodermic injection of morphia.

January 2, 1872.—Patient getting weaker and weaker. His diet consists of milk, wine, and hot whiskey. He has no control over his bladder or rectum, both urine and fæces passing involuntarily.

January 7th.—He died quietly at 10 A. M. to-day.

NECROPSY.

Twenty-four hours after death. *Rigor mortis* well marked; body emaciated; lungs and heart healthy; liver congested, substance pale, and on section smooth, hard, and mottled in appearance. Gall-bladder contained about half an ounce of dark-colored bile. Spleen rather larger than normal.

Kidneys.—The right was considerably larger than normal; upon section, cortex pale, pyramids partially obliterated; cavity of the pelvis filled with pus. The left was a mere sac filled with pus, all traces of kidney-structure having entirely disappeared; it contained a large, irregularly three-cornered, rough calculus, nearly as large as a black-walnut.

The ureters were greatly enlarged and thickened. The bladder was very much diminished in size, its walls thickened, the mucous membrane thrown into large folds or rugæ, and covered with dark-colored spots resembling ecchymoses. The prostate enlarged, consisting of two cavities containing pus, each opening into the rectum.

There are many reflections suggested by this painfully interesting case. It presents an example of the extensive changes in the urinary apparatus, extending to every part thereof, which sometimes follow neglected or overlooked stone in the bladder.

Diminution of the calibre of the bladder, irritation propagated through the ureters to the kidneys, damming up of urine in the pelves of these latter organs, leading to inflammation and destruction of tissue, with secondary calculous deposit from retained urine, with deterioration of the general health; these are a part of the catalogue of ills that wait sometimes upon calculus in the bladder. The best illustrations of these morbid conditions to which I can refer are to be found in Crosse's "Prize Essay on Urinary Calculus," where many cases resembling the one just described are figured. With regard to the failure of the wound of operation to unite, both that and the opening into the rectum are, I think, to be attributed to the condition of the patient's health. I am certain that the opening into the rectum was not made at the time of the operation; indeed, I do not see how it could have been without being detected at the time or shortly afterward; it probably occurred some time subsequently by ulceration, or was connected with the abscesses discovered in the prostate after death. In further confirmation of this idea, I quote from Crosse's "Prize Essay on Urinary Calculus," page 84: "The rectum is so contiguous to the urethra, that an opening may occur subsequent to the operation from sloughing, resulting from violence, or from ulceration in a bad constitution." With regard to the treatment of such a complication, he says, a little farther on, on the same page: "Where the perineal opening also remains, forming a recto-perinæo-urethral fistula, dividing the verge of the anus by an incision including the parts between the two openings, has been recommended; I once succeeded by this method." The general advisability of such a proceeding seems to be doubtful, at least to my mind. There is one strong objection to it: if the incision is carried through the *raphé* of the perinæum, as it almost necessarily must be, the central point of attachment for the converging fibres of the sphincter is divided, and the natural result will be a wide separation of the divided portions of the muscle, similar to what happens in lacerated perinæum, and perhaps finally necessitating a similar operation for its cure.

In the case under discussion this seemed to be the result; the edges of the wound were drawn widely apart, there was no tendency to heal, and the edges became firmly skinned over, so that they never could have healed without some further operation; but, perhaps, no inference can be justly drawn from one case, and *such* a case.

Notwithstanding the unfortunate issue of the case, it can in no way be attributed to the operation, which was useful in alleviating, though only temporarily, some of the distressing symptoms under which the patient labored.

CASE VIII.—Thomas Powers, aged seventeen years, born in New York; admitted October 26th. Father and mother both dead, cause of death cannot be ascertained. He has two brothers and two sisters; one of the sisters has phthisis pulmonalis, other sister and brothers in good health.

He was paralyzed on the right side when an infant, less than a year old. The history is very imperfect and obscure, but his present condition seems to indicate a condition designated by Hammond "diffuse cerebral sclerosis," the right side partially paralyzed, somewhat smaller than the left, the muscles permanently contracted, with mental imbecility, the tongue still deflected to the right. He was admitted to the hospital for paraphimosis, caused by manipulation of the penis; this was easily reduced, when the prepuce was found to be preternaturally long, and incontinence of urine existing, the urine flowing almost constantly without control of the patient.

Examination with the sound revealed the presence of stone in the bladder. Diagnosis of two vesical calculi was made, and on November 1st the operation of median lithotomy was performed, and two calculi about the size of walnuts and very irregular in form were removed. These calculi were singularly smooth and polished, and, being of a bright chocolate color, and marked with concentric rings, they presented an appearance best compared to polished agates; they consisted mainly of uric acid, and, what is somewhat singular, after the lapse of a few weeks, their color in a great measure faded, so that they are now of a pale-lavender tint.

At the expiration of twenty-four hours, the patient began to pass part of his urine *per urethram*, although most of the urine was passed through the wound for the next ten days, after which the urine all passed naturally, and the incontinence had disappeared.

At the end of a month he was discharged cured, and I have repeatedly seen him since, quite well except his paralysis. These two cases have strongly impressed me in favor of the median operation of lithotomy. I do not think it, any more than any other operation is, or ever will be, of universal applicability, but I do think that in suitable cases it has advantages which need only to be known to be appreciated.

Its advantages, as I estimate them, may be arranged under the following heads, viz.:

1. Ease of performance.
2. Freedom from danger.
3. Control of the bladder after the operation.

1. Having performed both this and the lateral operation, I unhesitatingly declare my opinion that the median is the easiest of performance, indeed, I may say *as easy* as any operation can well be.

Having been so forcibly struck with the superior ease of performance of the median operation, I was very naturally surprised at the following statement of Sir Henry Thompson, in his "little volume, but great book," entitled "Clinical Lectures on Diseases of the Urinary Organs," etc., where, on p. 165, he says: "I cannot tell you which of these two operations is the easier; if any thing, perhaps, the lateral." To this judgment, notwithstanding it is Sir Henry Thompson's, I must decidedly object, for my own experience of the two leads most emphatically to a diametrically opposite conclusion, and I recommend the median as very much easier of performance than the lateral operation of lithotomy.

With regard to the *technique* of its performance, however, there is a hint in this same book of Thompson's which I regard as valuable, and to which I wish to direct attention. As ordinarily described, the directions are, after having introduced the grooved staff and feeling the stone, and having made out the outlines of the prostate, to thrust a straight bistoury back, downward, through the soft parts, just in front of the anus, until its point enters the groove of the staff, and then cut upward and outward at one incision.

Another method, and that which I employed in my last operation, is to dissect down to the staff by making an incision an inch or more in length just in front of the anus, before engaging the point of the bistoury in the groove of the staff. Sir Henry says: "I prefer the other mode," i. e., the one just mentioned, *and so do I.* It is a fact which has come to my knowledge, that far better and more experienced operators than myself, operating by the first method, have failed to lodge the point of the bistoury in the groove at the first thrust, but have carried it, they knew not where; an accident that may with certainty be avoided by this preliminary incision. I have seen the operation performed in the way here recommended once since I did it myself, and then the operator, keeping the back of the knife toward the rectum, made his preliminary incision by cutting upward; this method is more or less awkward, and I can see nothing to recommend it.

2. *Freedom from Danger.*—There can, it seems to me, be no dispute on this point. Both the lateral and the bilateral are confessedly liable to danger from hæmorrhage. I have seen one case of the latter operation where the bleeding was troublesome, and heard of one where it was fatal, and every direction for the older or lateral includes cautions for avoiding this danger, but in the median this source of danger is almost abrogated; if it is *median*, there are no vessels to cut.

There is another immunity which this operation enjoys, that deserves mention. There is no division of the prostate, therefore, no danger of dividing it too freely, and thereby incising the perineal fascia and giving rise to deep or pelvic urinary infiltration. I believe this danger has been made too much of, but, still, if it exists at all for the other methods of operation, it certainly does not for the one which I am now advocating. Thus, to say nothing further, we fully justify our second claim, that this operation is freer from danger than the other methods of lithotomy; and—

3. The patient has full control of the bladder after the operation.

Great was my surprise to find that Sir Henry Thompson, in his volume on "Practical Lithotomy and Lithotrity," when comparing the advantages of the various methods of operating, makes no mention of this, in my opinion, enough of itself, *cæteris paribus*, to decide the question in favor of median lithotomy wherever it is applicable.

Those who have performed the other operations, and have had the case, and witnessed the inconvenience of the "lithotomy sheet," as it is called, would, I should have thought, have required no other argument for the median operation in proper cases. I say in proper cases, for, notwithstanding all I have said in its favor, and I think no one appreciates it more highly than I do, I do not believe that the operation of median lithotomy is applicable to every case; indeed, I think this has been a weak point with all lithotomists: they have advocated some favorite operation for every case, to the exclusion of all others, instead of studying and mastering all, and trying to adapt the operation to the case.

CASE IX.—*Abscess of the Head of the Tibia opening into the Knee-joint; Amputation; Recovery.*—Caroline Thompson, aged three years, native of Yonkers; admitted April 25th. She has had an abscess on the right leg somewhat to the outer side, and just below the knee; it first began seven months previously, and about four months ago it was opened; the opening still discharges, and presents the appearance of a sinus leading to diseased bone. She was placed under chloroform and examined; the probe did not detect diseased bone; the sinus was opened more freely and dressed with lint; the knee was slightly swollen. Syr. ferri iodidi and good nourishment were ordered.

May 5th.—The inflammation in the joint has increased very rapidly; the joint is swollen, reddened, and very tender. The child's general condition is bad; she is emaciating rapidly, appetite poor, suffers much pain, perspires profusely.

June 18th.—The patient is worse—starting pains in the joint. She was chloroformed, and the actual cautery applied to the front of the joint on each side of the patella; the limb was put on a sheet-iron splint, and a poultice applied. Severe and protracted ulceration followed the application of the cautery. Though improvement resulted from the application for a time, it was temporary, and the child's condition became very critical; she was very thin, perspired profusely, ate nothing, slept but little, and suffered great pain. A consultation of the hospital staff was called, and the unanimous opinion expressed was, that immediate amputation was advisable. Accordingly, July 26th, I amputated the limb just above the knee by skin-flaps, and circular cut through the muscles; very little blood was lost. The operation occupied one minute and forty seconds.

Upon examination of the joint, the synovial membrane and all the ligaments were found very much diseased; the cavity of the joint was filled with pus, the cartilage was abraded only in one spot, which corresponded to an opening leading to an abscess about the size of a filbert in the cancellous structure of the head of the tibia. Upon sawing the tibia longitudinally, in addition to the abscess, it presented evidences of osteo-myelitis.

The stump healed very rapidly, almost by first intention. It was kept dressed with carbolic acid, and there was scarcely a teaspoonful of suppuration altogether; the ligatures came away on the tenth day. The improvement in the general condition of the child after the operation was something wonderful to behold. She almost immediately began to gain in flesh and appetite, and became quite fat and cheerful.

She was discharged September 1st, in good health.

Case X. *Fatty Tumors.*—This case is only of interest from the number of lipomata existing in one person, and the unusual situation of some of them. Mary Murphy, aged fifty years, native of Ireland, seamstress; admitted October 6th.

She has a fatty tumor as large as a fœtal head just above and to the right of the umbilical region, another as large as a goose-egg on the right shoulder, two very small ones on the face, two as large as filberts on the back, one still larger and pendulous on the perinæum, and one on the verge of the anus.

The growths were all removed by operation, and, though the large wound caused by the removal of that on the abdomen was rather slow in healing, she made a good recovery, and was discharged cured.

II.—*A Rare Condition of the Red Blood-Corpuscles in a Case of Morbus Addisoni.* By Laschkewitch. [Med. Jahrb., 3.]

In a man of forty-five years, with pale skin, great œdema of the hands and feet, palpitation, great debility, dyspnœa, and headache, the objective symptoms were only a massive projection of the liver in the parasternal line and slight enlargement of the spleen. The illness began about a year before with diarrhœa, vomiting, and debility. During the observation, memory and intelligence became weakened, and convulsive contractions of the limbs and delirium set in. A microscopic examination of the blood in this obscure case showed that the red blood-corpuscles appeared paler and larger (flatter), threw out projections which were again withdrawn, contraction also, and consequent subdivision, was observed. Under a strengthening diet this condition somewhat improved. A second examination of the blood, after a month, showed "that the numerical relation of the blood-corpuscles had increased, each one was higher colored and the change of form continued." After three months the patient again presented himself with bronzing of the skin of the face, neck, hands, nipples, scrotum, and axillæ, when the diagnosis was fixed at morbus Addisoni only: 1. A stoppage in the portal circulation; or, 2. An affection of the peritonæum as cause of the ascites. As to the first the rare cases of thrombosis of the portal vessel are generally excluded, since they are preceded by rigors and followed by icterus, and their course is, besides, much more rapid. It is more difficult to exclude cirrhosis of the liver as cause of the ascites. The main points of the differential diagnosis are, that cirrhosis of the liver is the effect of the abuse of alcohol, or is caused by a malarial process; its course is apyretic, the abdomen becomes painful only when largely distended, a collateral venous circulation becomes established, the dulness over the liver is very slight, there is tumefaction of the spleen, frequent icterus, and the urine highly colored. If we exclude cirrhosis, we must explain the ascites by some affection of the peritoneal cavity itself. In the extraordinary rareness of simple chronic thickening and contraction of the peritonæum, it becomes more the question between tuberculosis and cancer of the peritonæum. Cancer is as a rule secondary to cancer of the liver, stomach, etc., and even primary cancer of the peritonæum develops itself without fever, with the formation of irregular tumors in the abdomen and irregular thickening of the walls, to which only later on, is ascites added. A retrocession, such as appears to occur for weeks in tuberculosis of the peritonæum, does not occur in cancer. Especial regard is to be paid to the occurrence of partition of the fluid effused through pseudo-membraneous bands, which phenomenon may also similate as ovarian cyst. Among remedial measures puncture is often necessary, and especially where, from the extent of the effusion, the adjacent organs are much interfered with and œdema of the lungs is threatened. During the continuance of marked peritoneal irritation, Kanlich discountenances the puncture; but where this is absent he does not delay long to relieve the respiratory and digestive organs, especially as it is only with a free respiration and cardiac action that the diuresis can increase. In the puncture, precaution is vised against the injury of some adherent band of intestine. Where evidences of blood in the effusion exist, the puncture is only to be resorted to as the last resource. In puncturing, especially the first time, the fluid should be allowed to run only so long as active contraction of the abdominal walls is evident. In pneumatosis, in consequence of immobility of the intestine, intestinal puncture might be needed.

Bibliographical and Literary Notes.

ART. I.—*A Practical Treatise on Bright's Diseases of the Kidneys.* By T. GRAINGER STEWART, M.D., F.R.S.E., Physician to the Royal Infirmary, etc., etc. Second edition, pp. 334. New York: William Wood & Co.

IN this edition Dr. Grainger Stewart does not modify his former opinions, but hopes that "much additional information, especially in respect of clinical history and treatment, may be found." No space is devoted to the chemical examination of the urine, anatomy, or physiology. After a short chapter on classification, the subject is at once entered upon. His views are more or less known respecting the amyloid form of these diseases, but his classification differs from that of other authors on renal diseases, and may not be familiar to all American readers.

While the author acknowledges it to be difficult to define Bright's disease, he says: "It may be generally described as including the diseases proper to the kidney, which are accompanied, at one stage or other of their course, by albuminuria, or dropsy, or by both." Three classes are given, viz.:

1. The inflammatory form, divided into the stage of (*a*) inflammation, (*b*) fatty transformation, (*c*) atrophy.

2. The waxy or amyloid form, divided into the stage of (*a*) degeneration of vessels, (*b*) secondary changes in the tubes, (*c*) atrophy.

3. The cirrhotic, contracting, or gouty form.

It will be observed that the usually-described fatty kidney is eliminated from the series, being considered under the head of inflammation. Perhaps a quotation from page 18 will give an idea of the author's view of the difference between inflammatory atrophy and cirrhosis:

"The pathological distinction depends mainly upon the condition of the tubules and the relative amount of connective tissue. When atrophy is a consequence of inflammation, many of the tubules exhibit evidence of the inflammatory action; they are occupied by exudation and epithelium in process of fatty degeneration, while in the cirrhotic there is little or none of this. . . . In the cirrhotic the fibrous stroma is greatly increased, while in the inflammatory form it is certainly relatively more abundant than in health, but by no means to such an extent. The capsule also is more easily stripped from the surface in inflammatory than in contracting cases, and cysts are more common in the latter than the former."

The different varieties are frequently associated in one stage or other, thus accounting for much of the confusion in the medical mind in regard to the different forms. It will be seen that the anatomical and pathological views of Virchow are accepted. The author does not seem to be aware of the existence of such a man as Beale—at least no mention is made of his recent anatomical investigations and pathological conclusions. What the author calls "connective tissue," "fibrous stroma," Beale considered altered vessels and wasted secreting structure. He (Beale) also believes that neither the intertubular connective tissue nor the vessels are usually the starting-point of any renal disease. The clinical history, so admirably described by the author, seems to confirm his views, and the simplicity of the arrangement rather commends its adoption.

It will be impossible to give even a synopsis of the points in the clinical differentiation of the different forms, either of the complicated or pure. A few may be noted. Neuro-retinitis, which is very common in the contracted form, is exceedingly rare in the inflammatory or waxy. Dropsy is most common with the inflammatory, with much albumen, tube casts, and diminution of the urine, except in the third stage; while polyuria, slight dropsy, and albuminuria, attend the waxy and cirrhotic forms. The waxy is shown to be the result of any exhausting disease like bone-affections, attended with suppuration, tuberculosis, and especially of syphilis. The cirrhotic is frequently associated with cirrhosis of the liver, gout, and lead-poisoning (after Garrod).

The author's view respecting the pathology of the waxy disease is already known to be that of *degeneration*, in opposition to the *infiltration* theory entertained by Budd, Portal, Rindfleisch, and Dickinson. This opinion is based mainly on the fact that the deposit first takes place in the middle coat of the small arteries. It cannot, therefore, be dealkalized fibrine.

The "nature of the symptoms" is briefly discussed in connection with each variety, presenting the views of many of the latest authorities. The knotty question of uræmic poisoning is not omitted, but no new light is thrown upon it. In regard to the "hypothesis" of Traube (œdema of the brain), it is stated on page 85:

"This hypothesis is certainly well worthy of being carefully investigated, for the condition of the brain met with in fatal cases of uræmia often accords with it, at least in the chronic cases. . . . But I have some difficulty in accepting it as explaining uræmic and acute cases."

The investigations of Oppler, and his conclusions confirmed by others, which have supplanted the theory of Frerichs, receive prominence. The products of muscle and nerve-waste, which are designed to be converted by the kidneys into urea, are supposed by Roberts,[1] Bashaw,[2] and others, to be creatine, creatinine, sarcine, and other extractives. The author leans toward, but does not support, the view of Argyll Robertson, that of "some forms of structural changes in the brain, similar to those which occur in the retina in cases of albuminuric retinitis." In regard to the mechanism of *convulsions*, Dr. Joseph Carson, in an able and searching review on puerperal eclampsia, in the *American Journal of the Medical Sciences* for April, 1871, among other conclusions, shows that convulsions are produced by loss of blood in the brain and by abstraction of nutrition in the motor portion of the brain; that anæmia predisposes to convulsions; that the blood of albuminuria and dropsy is deprived of its nutrient elements, and becomes hydropic; that the mechanism of convulsions involves the production of syncope, which depends upon the suspension of the heart's action, and that this occurs through the instrumentality of reflex action, by shock, or by loss of blood; that by the occlusion of the glottis the blood becomes venous, the carbonic acid stupefies the brain, and terminates the convulsion by inducing apoplexy. These conclusions are logical, and ingeniously deduced from the observations of a large number of experimenters, and have an important practical bearing.

In supplementary chapters, "simple fatty degeneration of the kidney" and "acute atrophy of the kidney" are treated. These are not considered as properly belonging to Bright's diseases. The article published in the *British and Foreign Medico-Chirurgical Review*, for 1867, on "The Complications of the Different Forms of Bright's Disease," is reprinted.

The most of our space has been devoted to the special views of the author. As regards the general character of the work, we regard it as eminently practical, and the lucid descriptions leave a clear impression of his views upon the mind of the reader. Perhaps more space is devoted to the report of cases than most men in busy practice care to read; still, we have felt benefited by studying them all. Many cases in the first and second stages of the inflammatory form are curable, while in both the other forms treatment does much to stay the progress of the disease, and benefit the patient's general condition. Frequent reference is made to Dickinson, Rosenstein, and other late authorities. The nine lithographic plates handsomely illustrate the text. C. H. R.

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[1] "Urinary and Renal Diseases," p. 359.

[2] "Renal Diseases," American Edition, pp. 278, 279.

ART. II.—*The Principles and Practice of Surgery.* By FRANK HASTINGS HAMILTON, A. M., M. D., LL. D., Professor of the Practice of Surgery, with Operations, and of Clinical Surgery in Bellevue Hospital Medical College; Visiting-Surgeon to Bellevue Hospital; Consulting-Surgeon to the Bureau of Surgical and Medical Relief for the Out-door Poor at Bellevue Hospital, to the Central Dispensary, and to the Hospital for the Ruptured and Cripples; Fellow of the New York Academy of Medicine, etc. Illustrated with Four Hundred and Sixty-seven Engravings on Wood. New York: William Wood & Co., 27 Great Jones Street, 1872.

WE have before us a new text-book on surgery, from the pen of one of our most accomplished and widely-celebrated professors of the "art and science," which he introduces to our acquaintance in the following appropriate words: "It was the purpose of the author, in the preparation of this work, to supply, within the compass of a single volume of moderate size, the instruction necessary to a full understanding of all the subjects belonging properly and exclusively to surgery; the volume being intended as a text-book for students, and at the same time as a direct and complete guide to the surgeon. To this end, each department has been treated with as much conciseness as was consistent with precision and completeness.

Much of the literature of surgery has been omitted; questions which affect only remotely the conduct of the operator have been discussed with brevity, and there has been substituted, wherever it seemed necessary to a thorough comprehension of the subject, a more minute description of the surgical anatomy and of the most approved operative procedures than is usually found in similar treatises."

To the fulfilment of the promise thus made, the learned and conscientious author has brought forth the stores of a long life of careful study, and extensive and diversified practical experience, methodized and disciplined by the constant practice of giving didactic instruction, and the still higher and more severe tests of honest and fruitful labor in voluminous and successful authorship. His weight and consideration in the profession entitle this new work to the most respectful reception, and dispose us to accept with pleasure and satisfaction from his hands what we should otherwise consider almost a superfluity, in view of the numerous recent works upon the same subject written from the same point of view.

It is divided into two parts, to wit:

General Surgery, and Regional Surgery; and opens classically with a chapter on inflammation, the corner-stone of disease, as healthy nutrition is the corner-stone of health.

Prof. Hamilton has presented a clear, beautiful, concise, and intelligible view of inflammation, its results and consequences, and the treatment to be adopted with reference to their varying and complex aspects and conditions, which we consider for practical purposes the best we have ever read. We find nothing to add, amend, or dissent from, in the aphorisms, principles, or directions, uttered and laid down. We recognize in the writer a thorough comprehension of the subject, and a clearness, force, and simplicity of expression which will enable every student, and those practitioners who are clouded and mystified by an effort to wade through the learned literature of this department, to grasp the principles and apply intelligently the best methods as adapted to every case. *A propos* to this portion of the work, the principles of transplantation and skin-grafting, and all that is at present known upon this real and substantial advance in surgery, are given and lucidly explained. Under the head of Gangrene, we are glad to see the claim of Dr. Middleton Goldsmith to the first application of bromine in hospital-gangrene placed on permanent record in a systematic work.

We regarded, at the time, and still believe, that Dr. Goldsmith's discovery was of immense value, and contained within it the nucleus of the whole system of the antiseptic treatment of wounds.

The author gives an excellent outline of the history and clinical facts of tetanus, but adds little to our present knowledge. It would seem by analogy that electricity would offer more hope as a rational and successful remedy than any other which has hitherto been proposed.

We would recommend also, in this disease, a fair trial of large doses of the bromide of potassium and ammonium in combination.

The chapters on traumatic tetanus, wounds, arrow-wounds, and gunshot-wounds, with their varieties, are exceedingly interesting, and leave little to be desired in the way of didactic outline. Prof. Hamilton has drawn largely from his own invaluable experience and the records of our own and other wars. In speaking of wounds of the abdominal viscera, he mentions two cases of recovery from gunshot perforation of the stomach. We add a third out of our own experience, that of Dr. R. B. Cole, of San Francisco, whose case was reported in the *American Journal of Medical Sciences*, by Dr. Fechter, late surgeon U. S. A. The directions in regard to wounds of the liver are not sufficiently clear and explicit, and might easily betray an inexperienced surgeon into error. We apprehend the author's meaning, which he has made clear and explicit in his work on "Military Surgery." Probing of the liver, if required to remove foreign substances, such as fragments of clothing or pieces of comminuted rib, should be done with extreme caution by the fingers or a probe with a pliant rounded end. In any case too much caution cannot be observed. The descriptions and recommendations are singularly clear, lucid, direct, and instinct with practical and enlightened common-sense. Dr. Howard's method in regard to wounds of the chest receives due attention, and its application is justly discriminated between the cases where it is valuable, and perhaps imperative, and those conditions in which it is either tentative or useless.

Chapter XIII. contains an excellent summary of the doctrines and practice entertained and pursued by the leading surgeons in this city, in the several forms of venereal diseases which form the basis of the treatment practised in Charity Hospital and Bellevue. Each succeeding year, with its new cases and returning old ones, adds confirmatory evidence that these doctrines are founded in pathological truth, and developed and made practical by correct study and observation. Dr. Hamilton gives more credit to syphilization than we think it deserves, and this is the only point in which we dissent from his pathology or the principles of his practice.

Lesions of the vascular system and aneurism compose two chapters. They are briefly, clearly, and accurately stated and discussed. The new methods of closing vessels by acupressure, torsion, and constriction, are appreciated, and intelligibly described and illustrated. Spier's artery constrictor, which we regard as a valuable instrument, is given its permanent place in the surgical category, and Dr. J. W. Smythe's great case is placed on record, we believe, for the first time in a systematic work. We have often wished that some good angel would communicate to Dr. Mott, in the other world, the fact that the innominata had been successfully tied in America.

Our learned author, confessedly one of the highest authorities on dislocation and fracture, is of course thoroughly at home in these departments of his general work, and little can be said in the way of additional commendation or criticism. We do not think, however, that he gives full weight to Dr. Moore's idea in regard to the complications of fracture of the lower end of the radius. In the directions for treatment of fracture of the femur, no mention is made of the plan of counter-extension by plaster-bands from the body of the patient, and over the point of the shoulder, with extension by means of the screw. This method, which has been described in the *Medical Record*, seems to us the best for making extension and counter-extension. Traction is exerted exactly in the long axis of the limb; the easiest position for the patient is with the axis accurately preserved, and bandages and the perineal band are avoided. Clear directions and good illustrations of dislocations, and the proceedings necessary to reduce them, either by extension or manipulation, are given, coupled with the cautions necessary to the inexperienced.

Amputations and excisions are treated from the light of modern surgery. We accept with pleasure the author's dictum in regard to excision of the knee-joint, which is nearly identical with our own views and experience. Prof. Hamilton sets his face against surgical tragedies. The summary of diseases of joints, and the points of value in treatment, are satisfactory, and the illustrations furnish a correct guide in the selection and application of the various mechanical contrivances for relieving inflamed, tender, and opposing articular surfaces from contact and friction. It is always to be borne in mind that these varieties of apparatus are of the nature of crutches, are means toward an end, and are liable to misapplication and abuse.

Our author is especially conservative in his recommendation as to the treatment of the common and troublesome condition, enlarged bursæ patellæ. We have been in the habit of treating them by the seton until suppuration made its appearance, and then opening the abscess, with subsequent application of glycerine and carbolic acid. We have hitherto had no cause to regret or condemn this method. The chapter on tumors is excellent, and the additional observations made under the head of regional surgery are sound, and in harmony with the advanced pathology of the present day. The chapter on concussion and contusion of the brain, and compression with or without fracture of the skull, is highly sensible and conservative. The distinctions are clear and well drawn, and, in the main, all agree with the laws laid down in respect to surgical interference and subsequent treatment. Hernia, in all its varieties, is briefly but well discussed. Dr. Hamilton recommends the ice-bag as a mode of taxis and pressure, and simultaneous prevention of contractility through the influence of cold. We recommend, as a substitute—safer, more elegant, and efficacious—the distillation of the spray of absolute ether, which will accomplish rapidly and without danger, either from pressure or otherwise, all that can be obtained from cold, and more speedily decide the point between reduction and operation.

The surgery of the rectum is treated intelligently and well, and the directions given are safe, correct, and intelligible.

The genito-urinary organs, their defects, accidents, and surgical diseases, together with operations and manipulations, are contained in the seventeenth chapter, and are evidently studied *con amore.* We approve and indorse most of the opinions and directions of the learned and cautious author, but we regret that he has not considered the method proposed by Dieulafoy, of relieving the over-distended bladder, when puncture becomes imperative, by the capillary trocar and aspirator, or the trocar alone. Prof. Hamilton recommends and illustrates puncture *per rectum* and *supra pubis*, by means of the old-fashioned curved trocar and canula. We regard the latter method as having been rendered obsolete by the new, improved, and safe proceeding adverted to above. Dieulafoy's operation has received the sanction of eminent surgeons; it appeals directly to analogy and common-sense, is safe, easy, and trustworthy, and, in our judgment, should be adopted as the operation in all the cases in which puncture is required. We are constrained to note, as an omission in the department of urethral surgery, the absence of any mention of the improvements, both in instruments and method, of Dr. Gouley, one of Dr. Hamilton's colleagues in Bellevue Hospital. Dr. Gouley's studies in this department are among the most excellent contributions to this department of surgery, and are entitled to recognition in the standard literature of the country. We are sorry, also, not to see an illustration of Dr. Otis's admirable dilator and urethrotome for strictures of extreme calibre. We regard Dr. Otis's contributions to this subject as exceedingly valuable. Dr. Hamilton recommends the retention of a catheter in the urethra, after external urethrotomy, for twenty-four hours. To this we dissent, as a rule. Exceptional cases may suggest its expediency to the mind of an operator.

The surgery of the female genito-urinary organs concludes the book. A large amount of space is devoted to Dr. Bozeman's operations, instruments, and modes of practice.

Prof. Hamilton makes a claim for Dr. Bozeman which we think will hardly be allowed to pass without challenge by other surgeons who have made this department of surgery the business of their lives. No reference is made to Dr. Emmet's work on this subject, in which he has demonstrated his extraordinary success in extreme and difficult cases.

In conclusion, we do not hesitate to pronounce the work, which we have briefly reviewed, a valuable addition to our list of text-books, an excellent work of reference, and, in most respects, a sound, safe, and trustworthy guide to practitioners. It is clear, terse, forcible, even if at times somewhat dogmatic; the latter quality is more or less inseparable from an original mind of strong convictions, corroborated by the results of great experience and the habit of teaching, writing, and acting, as a consulting authority. The work is a credit to our professional literature, and demonstrates that we can make books, and good ones, as well as edit them. The plates are excellent, and well selected. A practical work on surgery cannot have too many good illustrations.

We congratulate the learned author, whom we are proud to be able to number among our friends, upon his latest contribution, which sustains what may justly be called a cosmopolitan reputation.

ART. III.—*A Treatise on the Diseases of Infancy and Childhood. Second edition, enlarged and thoroughly revised.* By J. LEWIS SMITH, M. D., etc. Philadelphia: Henry C. Lea, 1872, pp. 741.

DR. SMITH's treatise is well and favorably known, both in this country and in Europe. The new edition contains upward of a hundred more pages than its predecessor, although the size of the page is increased. Among the added matter may be mentioned the diseases incidental to birth, rachitis, tuberculosis, scrofula, intermittent, remittent, and typhoid fevers, chorea, and the various forms of paralysis.

The prolegomena (which we always expect to find in a treatise on the pædiatrics, and which are so apt to prove tedious), referring particularly to the peculiarities of structure and function in the infant, to the accidents at birth, to dietetics, and to general considerations upon the hygiene of infancy, and the diagnosis and pathology of infantile diseases, are very well presented by Dr. Smith, and occupy no more than their due amount of space. Within a few recent years, the opinion has gained great credence in the profession, that the enormous mortality from the diarrhœal diseases of infants is largely due to that particular form of improper feeding which includes the free use of amylaceous food during the early months of life. It is to be regretted that Dr. Smith has not discussed the matter in a general way more fully in this volume. Incidentally, it is true, he deprecates the use of starchy food during the months preceding dentition, but we fail to find evidence of a decided conviction of the grave consequences to be attributed thereto. In the absence of any such conviction on the part of Dr. Smith, his readers would have attached the highest value to a *résumé* of the facts and alleged facts which make for and against the opinion—a *résumé* which, by-the-way, Dr. Smith's style of writing and praiseworthy fairness of reasoning would have made very clear and explicit.

From the author's reputation as a pathologist, we should naturally look for a trustworthy and well-constructed account of lesions and morbid processes, and, in the main, this promise will be found fulfilled. We may refer the reader particularly to the chapter on tuberculosis. Collapse of the lung does not, however, receive that prominent consideration which should have been devoted to it, and the masterly presentation of which forms so marked a feature in another recent American contribution to the literature of children's diseases—the admirable treatise of Drs. Meigs and Pepper.

The author's devotion to pathological anatomy has not, however, prevented him from treating of children's diseases very satisfactorily from a clinical point of view; and this is not only very much to his credit, but also of great value to his readers, as giving them (as, happily, medical writers of the better sort are more and more getting into the way of doing) a picture which they may read, rather than a scheme which they shall feel bound to make facts conform to.

As the volume before us is not, as a whole, new, we have purposely refrained from particular criticism of the author's consideration of individual diseases. We may, however, say, in general terms, that the work embodies a well-constructed exposition of the present state of our knowledge of the diseases of early life, pervaded, as it should be, with the results of the author's own extensive and careful investigations. It forms a trustworthy text-book for the student and the practitioner. A particular merit lies in the fact that, in lieu of the commonplace advice of authors to remove the patient *to the country* in certain cases of disease, special localities are mentioned as to be recommended for this or that sort of case.

As regards the mechanical appearance of the volume, it accords with that of Mr. Lea's numerous medical publications.

The index is somewhat meagre and unsatisfactory, but this is of little consequence to the systematic reader.

ART. IV.—*The Abuse of Alcohol in the Treatment of Acute Diseases. A Review.* By T. P. HESLOP, M. D., Physician to the Queen's and Children's Hospitals, Birmingham. London: J. & A. Churchill, pp. 39, 8vo, 1872.

THE above is the title of a recent publication in book-form of a criticism of Dr. Todd's "Clinical Lectures on Certain Acute Diseases," in the *Dublin Quarterly Journal of Medical Science*, in 1860. It comes to us, after the lapse of twelve years, without revision or modification.

It may be remembered that Dr. Todd took an open stand against the hitherto-prevailing practice of treating phlegmasiæ by the usual antiphlogistics—venesection, mercury, and antimony—and showed that a supporting treatment was unattended with danger of intensifying or prolonging the local mischief. It is not surprising that some theories should be advanced which would not stand the test of twelve years' investigation. When great and important changes in opinion are being brought about, the opposite extreme is a danger which all have not avoided. Dr. Heslop professes merely to oppose the undue use of stimulants, and keep the passions of the profession within due bounds; but the reviewer must be cautious lest he be found travelling in a rut. It may be inferred, from the criticisms upon the treatment adopted in some of Dr. Todd's cases, that the author under present review would limit the use of alcohol to a precious few cases.

It is not our purpose to discuss the nature of fever, inflammation, or the action of alcohol, but will give expression to the opinion that there is nothing in strength of constitution or vigor of system incompatible with perfect health; and the nearer we can bring the individual, suffering from any affection, to a healthy standard, the more readily may diseased action be displaced. We agree with Dr. Todd in believing that no diseased action exalts the vital powers; and, if this action is incompatible with the *vital resistance*, stimulants may at least uphold the powers of the system until it has time to recuperate.

It is stated, on page 8 (after Symonds), that in the early stage of inflammation attended with fever there is no waste to be repaired, nor vital power to be upheld, as is shown by the heart's action being abnormally strong, by the respiration being abnormally excited, and by the nervous sensibility being already too acute.

We had supposed an elevation of the temperature of the body was attended with a corresponding exhaustion of vital power. The increase of urea found in states of high fever, with or without inflammation, may be supposed to indicate a waste of tissue beyond physiological limits, urea being formed in the kidneys from products of muscle and nerve waste.[1]

The author criticises Dr. Todd's method of treating pneumonitis because he lost two out of eleven cases.[2] One of the fatal cases was "laboring under acute rheumatism, double pleuro-pneumonia, and endo-pericarditis." The other was a case of single pleuro-pneumonia in a patient forty-eight years old, complicated with kidney-disease. In the former case, on the twelfth day of the disease, three ounces of wine was administered, the quantity being gradually increased to eight ounces daily. In the latter, half an ounce of wine was ordered every four hours from the fifth until the eighth day, after which half an ounce of brandy was given hourly until he died, on the eleventh day of the disease.

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[1] Oppler; Zalesky; *vide* "Bright's Diseases," by T. Grainger Stewart. American edition, pp. 87, 88.

[2] The author excludes one case, on account of being associated with typhoid fever, reducing the whole number to ten.

We confess that we are too obtuse to appreciate that alcohol contributed to the fatal termination in these cases. Although we more nearly agree with our author in his criticism of the *amount* of alcoholic stimulants used in typhoid fever, and some cases of rheumatism, by Dr. Todd, his views upon the use of this article and opium in the brain complications of these affections, as being set forth in 1872, show a want of respect for, or appreciation of, the more recent investigations, and received opinions. In the treatment of fevers and inflammations, in addition to specific treatment,[1] the profession seem pretty well united in administering nourishment early to supply waste, adding the extra horse on arriving at the hill, if a steep one. The patient (as well as the disease) is treated with a view to avert the most pending evil. In pneumonitis affecting the superior lobes, bleeding may be resorted to, to mechanically relieve the lung and save the patient from impending suffocation, but not to cure the inflammation. If (as in some inflammatory affections of the vital organs) death is threatened from asthenia, support in due season is indicated, always keeping within the toxic action of any remedy employed.

In this country this little work is not calculated to do harm, perhaps good. In the midst of an atmosphere tinctured by the writings of several of our late recognized authorities, stronger arguments than Dr. Heslop's will be required to bring back the old antiphlogistic (?) practice. C. H. R.

ART. V.—*Diseases of the Throat.* By J. SOLIS COHEN, M. D. William Wood & Co., pp. 582, 1872.

THE first 332 pages of this work treat of diseases of the pharynx, tonsils, œsophagus, nasal passages, frontal sinus, etc.

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[1] We have elsewhere expressed the opinion that opium is "antiphlogistic," by arresting exosmosis and endosmosis.

Following this to the 507th page, the diseases of the larynx and trachea are set forth; while the remaining pages are devoted to the study of "diseases of the neck affecting the deeper tissues of the throat secondarily."

The whole book is very complete, and contains a great deal of original matter. The pathology and treatment as given by others are fully stated, and free, honest criticism on the same, with many practical suggestions, is given by the author.

On page 297 a "submucous infiltration at the sides of the vomer" is described as one source of obstruction of the posterior nares. The author thinks this has not previously been specially described, and we do not remember to have seen it mentioned. The treatment advised is tearing away portions of the protrusions by forceps introduced through the nares or from behind the palate.

On page 5 the author calls attention to the influence of tobacco in inducing chronic irritation of the throat. Though, from the leniency of his remarks, we may imagine he is a smoker, he seems to recognize the occasionally pernicions influence of the habit. Throughout the book the use of sprays of various kinds is generally preferred to gargles, they being more likely to come in direct contact with the parts afflicted. But it has often seemed to us that gargles were useful from the gymnastics of the throat induced by them, and that a certain amount of infiltration of the pharyngeal mucous membrane was removed by the action of the muscle. On page 447 the invention of hooks for use in thyrotomy is ascribed to Dr. Cutler instead of to Dr. Cutter.

In diphtheria the spray of dilute sulphurous-acid water is highly spoken of.

In several places the author asserts the greater liability of anæsthetics to prove fatal when given for the *prevention* of pain than when administered for its *relief*, and on page 449 even proposes that on this account the preliminary incision in laryngotomy should be made before anæsthesia. It is perhaps going somewhat into the region of speculation to discuss such a question. But it might be suggested that, when given for the *relief* of pain, the anæsthetic is not pushed to the same extent as when given for the *prevention* of suffering and the relaxation of the muscles.

This book is very creditable to both author and publisher; and treating, as it does, of affections of such daily occurrence, it must prove acceptable to the profession.

ART. VI.—*The Mode of Propagation of Cholera in India.* By PETTENKOFER. [Braunschw., 1871, and Rindsch., 1872, p. 392.]

THIS work shows us how little we know of the true nature of cholera. The author gives us here the views of the most eminent investigators, and, although he critically dissects these and strives to supplement them with his own views in order to finally fix the etiology of cholera, he only partially succeeds in convincing either himself or the reader of the final correctness of these views.

Nevertheless, this work of Pettenkofer deserves the full attention of the faculty; for, not only are we in a state of infancy as regards epidemics, and more, perhaps, than ever before, ready to adopt new views on the subject, but we find in this work veritable stand-points indicated, from which the investigation of the etiology of cholera will eventually be carried out with better effects. We will cite the more important points. Pettenkofer makes known to us the views of cholera as entertained in India; the miasmatists, among whose followers is Bryden, there stand entirely opposed to the contagionists represented by Macnamara. Bryden turned his attention principally to the garrisons and prisons, and, on the basis of his observations, prepared charts on the diffusion of cholera from the years 1855 to 1869. It results from these charts that the regions near the mouths of the Ganges and Brahmapootra are endemic sources of cholera. As the cause of cholera, Bryden sees the monsoon (rain-wind), which then prevails in those localities; the cholera always comes with the monsoon, although its germs may lie a long time dormant in the earth without necessarily coming to an outbreak. The cholera, says Bryden distinctly, is a miasm and not a contagion which is communicable from one person to another. This view is generally held in India by the masses, whence the inhabitants, upon the outbreak of cholera, always leave the infected spot, but at the same time do not fail to carry all cholera-patients with them. Macnamara adduces, in favor of his views, the so-called Hurdwar cholera of the year 1867. Hurdwar is a pilgrims' resort, where yearly in April, on a surface of a German mile, about three millions of men collect together. The cholera broke out among these people exposed to the greatest misery and living shelterless and in filth, and extended hence in the same directions taken by the pilgrims on their return homeward.

In answer to this, Bryden says, "No mass of pilgrims can produce cholera, or be attacked with it, except where cholera exists beforehand, and is ready for epidemic diffusion." Pettenkofer sums up these views as follows: "The cholera among the pilgrims, in my opinion, proves in fact only that individuals leaving an infected spot fell ill of cholera on the journey in consequence of an infection already existing at the locality, and that this return may possibly contribute to the diffusion of the disease in such localities and regions where the timely and local disposition is present, but not elsewhere." The noxious influence of the return upon the diffusion of cholera is contradicted by the Kuli transports, which, occasionally attacked with cholera, proceed to cholera-free tea-plantations without developing the cholera there. Positive evidence is afforded by the fact that the hospitals for cholera-patients are not a source of contagion. No great significance is attached to the quarantine in India, and Bryden says, "The cholera does not transgress the limits of its natural provinces, whether quarantine is imposed or not." Macnamara, however, advises the sulphate of iron and carbolic acid.

The drinking-water theory cannot be brought into question in the cholera in India. For while, indeed, in many cases, apparently, the drinking-water was the cause of the disease, the latter also occurred elsewhere where only cooked and filtered drinking-water was used. As to the incubation, it appears that the virus may have resided two and a half to five days, on an average three days, in the organism, before the outbreak. The inhabitants avoid those localities where cholera shows itself, and the Europeans also follow their example;

nevertheless, this is at times only injurious, especially in those cases where the spot was quitted too late, or where the locality to which the person goes is susceptible of the cholera-germ. Such susceptibility cannot so far be predicted beforehand. Ships, continues Pettenkofer, are also to be regarded as the means of transmission, only with the constant distinction between transmission on land, that ships are always to be regarded as places of immunity, to which indeed infected persons, and, in fact, by a certain compression, even infectious material in the ripe and unripe state may be carried from the land, but a ship can never sustain the *rôle* of the earth in relation to the cholera process, as little as the passengers thereon; the cholera on ships is always produced by and dependent upon previous influences and processes from the land. Europeans suffered equally and even more from cholera than the inhabitants, where both live under the same conditions; the individual disposition is about thirteen times greater in Europeans than in natives. The surest bases for the etiology of cholera are presented by local position and condition. According to Bryden, much more depends upon the different division of the atmospheric influences produced by the monsoon which drives over certain strips of land and certain years. Furthermore, Bryden, as well as Pettenkofer, assumes a certain abnormal condition of the earth for the infection of cholera, designating the disease as *earth-born* and *air-conveyed.* Finally, Bryden distinguishes the summer or monsoon cholera, which passes from the endemic to the epidemic localities, as distinct from the spring cholera, which is indeed primarily brought by the monsoon, but whose germs lay dormant in the earth until the outbreak. He calls the first, also invading the latter, revitalized cholera.

In favor of Pettenkofer's theory, speaks an interesting case which was observed by Dr. Cunningham in India. It happened in the year 1870 that in Rajmahal, an endemic cholera locality, cholera broke out. Rajmahal consists of two bazaars, both of which lie close to the banks of the Ganges, and are a half-mile apart. While in one of the bazaars cholera ravaged, the other was exempt, although the inhabitants lived in every respect under the same conditions. Further investigations made it evident that the infected portion of the town was built upon fragments of pottery and bones, while the healthy portion had an impermeable clay stratum, about five feet below the surface. Dr. Cunningham hereupon remarks that the impermeability might cease in time by the stratum losing its water; Pettenkofer also does not deny this, but believes that a great and long-continued dryness would be needful therefor. What the monsoon is with Bryden, the subsoil-water is with Pettenkofer. With both the effective influence of the atmospheric moisture is characteristic, although by different interpretations. For the existence of cholera the variation of the mass of subsoil-water is necessary; as Pettenkofer explains this by the increase of cholera during the greater dryness in the delta of the Ganges. Here the monsoon never reaches, and hence this phenomenon is unexplainable by the monsoon theory. Pettenkofer asserts the following as fixed facts based on observations partly in Europe, partly in India: 1. In India there exist certain localities in which cholera has been endemic from the earliest times. The cause of this endemicity is not to be sought in the people there inhabitant, but must be looked for in unexplained relations between the ground, the climate, and the germ of the disease. 2. The epidemics spread from these endemic regions, according to the miasmatics, through the monsoon; according to the contagionists, however, principally through the excrement of cholera-patients. In Europe, currents of air are never the disseminators of cholera; it is all the more the unhindered intercourse which, however, produces epidemics there only where the local and timely conditions are present. 3. The cholera-germ is capable of transportation by man; its development depends on certain relations of the soil. 4. Persistent drought, as well as great moisture of the earth, is unfavorable to cholera, but a certain grade of moisture is necessary to its production. 5. Upon individual disposition the number of patients is essentially dependent. In natives the predisposition is much less than with emigrants. Of the natives the inhabitants of the mountains have a greater predisposition than the dwellers in the plains.

Ships are to be regarded as possessing immunity; the ship can never replace the soil which is necessary for the development of the cholera-germ; only developed germs can produce epidemics on ships. 7. The indulgence in different and impure drinking-water, even that containing dejections from cholera-patients, cannot explain the local and chronic appearance of cholera in India. Finally, Pettenkofer is of the opinion that the near future will contribute much that is valuable regarding the condition of the soil, porosity, and impermeability; furthermore, continued investigations should be made on the variations of the water-level, temperature of the soil, amount of rainfall, and temperature of the atmosphere wherever the frequency of the cholera is studied.

Books and Pamphlets Received.—The Science and Practice of Medicine. By William Aitken, M. D., Edinburgh, Professor of Pathology in the Army Medical School. Third American, from the sixth London edition. Greatly enlarged, remodelled, carefully revised, and many Portions rewritten; adopting the new Nomenclature, and following the Order of Classification published by the Royal College of Physicians of London. With Additions by Meredith Clymer, M. D. (University of Pennsylvania), ex-Professor of the Institutes and Practice of Medicine in the University of New York, etc. In two volumes, with Steel Plate, Map, and One Hundred and Eighty Woodcuts. Philadelphia: Lindsay & Blakiston, 1872.

Practical Lessons in the Nature and Treatment of the Affections produced by the Contagious Diseases, with an Account of the Primary Syphilitic Poison and of its Communicability, based on Extensive, Direct, and Comparative Observations of the Diseases in Both Sexes. With an Appendix on the Recent Report of the Royal Commission on the Contagious-Diseases Act. By John Morgan, A. M., M. D., Fellow of the Royal College of Surgeons, Ireland; Surgeon to Mercers' Hospital, etc., etc. Philadelphia: J. B. Lippincott & Co., 1872.

Epidemic Cerebro-Spinal Meningitis. With an Appendix on Some Points on the Causes of the Disease, as shown by the History of the Present Epidemic in the City of New York. By Meredith Clymer, M. D. (University of Pennsylvania), Fellow of the College of Physicians of Philadelphia, Professor of the Diseases of the Nervous System and of the Mind, etc., etc. Philadelphia: Lindsay & Blakiston, 1872.

The Treatment of Syphilis with Subcutaneous Sublimate Injections. By Dr. George Lewin, Surgeon-in-Chief of the Syphilitic Wards of the Charité Hospital, Berlin. Translated by Carl Proegler, M. D., Late Surgeon in the Prussian Service, and in the United States Army, and E. H. Gale, M. D., Late Surgeon United States Army. Philadelphia: Lindsay & Blakiston, 1872.

On Some Affections of the Liver and Intestinal Canal, with Remarks on Ague and its Sequelæ, Scurvy, Purpura, etc. By Stephen A. H. Ward, M. D., London, Fellow of the Royal College of Physicians, Physician to the Seaman's Hospital, Hospital for Diseases of the Chest, etc. Philadelphia: Lindsay & Blakiston, 1872.

The Journal of the Gynæcological Society of Boston. A Monthly Journal devoted to the Advancement of the Knowledge of the Diseases of Women. Edited by Winslow Lewis, M. D., Horatio R. Storer, M. D., and George H. Bixby, M. D. Vol. VI. January to July, 1872. Boston: James Campbell.

General and Differential Diagnosis of Ovarian Tumors, with Special Reference to the Operation of Ovariotomy; and Occasional Pathological and Therapeutical Considerations. By Washington L. Atlee, M. D. With Thirty-nine Illustrations. Philadelphia: J. B. Lippincott & Co., 1872.

On the Functional Diseases of the Renal, Urinary, and Reproductive Organs, with a General Review of Urinary Pathology. By D. Campbell Black, M. D., L. R. C. S., Edinburgh, Member of the General Council of Glasgow, etc., etc. Philadelphia: Lindsay & Blakiston, 1872.

A Manual of the Diseases of the Eye. By C. Macnamara, Surgeon to the Chandnie, and Ophthalmic Hospital, Calcutta; Professor of Medicine and Surgery in the Calcutta Medical College. Second edition. Philadelphia: Lindsay & Blakiston, 1872.

A Hand-Book of Post-mortem Examinations and of Morbid Anatomy. By Francis Delafield, M. D., Curator to Bellevue Hospital, Pathologist to the Roosevelt Hospital, etc., etc. New York: William Wood & Co., 1872.

New Treatment of Venereal Diseases, and of Ulcerative and Syphilitic Affections, by Chloroform. Translated from the French of Dr. A. A. Izard, by Howard F. Damon, M. D. Boston: James Campbell. Pp. 73.

Transactions of the Medical Society of the State of Pennsylvania at its Twenty-third Annual Session held at Franklin, Pa., June, 1872. Vol. IX. Part I. Published by the Society, 1872. Pp. 249.

The Abuse of Alcohol in the Treatment of Acute Diseases. A Review. By T. P. Heslop, M. D., Physician to the Queen's and Children's Hospitals, Birmingham. London: J. & A. Churchill, 1872.

Evolution of Life. By Henry C. Chapman, M. D., Member of the Academy of Natural Sciences, Philadelphia. Philadelphia: J. B. Lippincott & Co., 1872.

Facts of Vital Statistics in the United States. With Tables and Diagrams. Extracts from an Address by J. M. Toner, M. D. 1872.

Normal Ovariotomy. By Robert Battey, M. D., Rome, Ga. Atlanta: Plantation Publishing Company, 1872.

A Successful Case of Ovariotomy. By George Holmes Bixby, M.D. Boston: James Campbell, 1872.

Transactions of the Indiana State Medical Society, 1872. Twenty-second Annual Session. Pp. 177.

Transactions of the Minnesota State Medical Society. 1872. Pp. 120.

Reports on the Progress of Medicine.

THEORY AND PRACTICE.

1.—*The Influence of Belladonna on Sweating.* By Sidney Ringer, M. D. [Practitioner, August, 1872.]

The remarkable influence of belladonna applied to the breast, in checking the secretion of milk, led the writer to try its influence on sweating. He first employed belladonna in a case of unilateral sweating. A man, forty-five years old, had been troubled for many months with very profuse sweating of the right side of the face and neck, breaking out on the slightest exertion or excitement, or when near a fire, so that the sweat ran down his face and neck in streams, soaking his collar and the band of his shirt, his face being neither red nor congested. The perspiration produced an abundant crop of miliary vesicles, which were strictly limited to one-half of his face. The liniment of belladonna applied two or three times a day abated this copious sweating considerably, and reduced it to a little more than the natural amount, and this improvement lasted about six months after the discontinuance of the application, and then the sweating gradually returned, till it became as bad as ever.

The writer has many times checked the sweating of the head and face of young children, often so profuse as to soak their hair and the pillow on which they have been sleeping. Again, belladonna, by means of the ointment or liniment rubbed in two or three times a day, has several times checked the profuse sweating of the hands, which is sometimes so copious as to run off them in drops, and is especially noticeable at the finger-tips and thumb-balls. Sometimes the good effects are permanent, sometimes the sweating may not return for a considerable time; but occasionally, however, this treatment fails.

The following curious case of unilateral sweating illustrates the beneficial effects of belladonna: Mrs. P., aged thirty, married, has all her life sweated freely, but much more on the left than on the right side of her body, the excess being most marked on the head and trunk, although the left arm and leg sweat more than the right. The line of demarcation on the face is sharply defined, equally dividing the head down the centre. The sweating on the left side is very profuse, running down her face and soaking her hair and even the bed-pillow. The sweating on the left side of the trunk is most marked, as low as the breast. Slight exercise, sleeping, or exposure to the heat of the fire or sun, especially the latter, greatly augment the sweating. It is markedly profuse when she is out of health. The sweating is unaccompanied by flushing, and does not excite any rash. She suffers from great coldness of the feet, but the right foot is decidedly the colder. She never feels the left hand warmer than the right. The left side of her tongue is always more coated than the right, and has been so as long as she can recollect. There are no bad teeth or other causes in the mouth to account for this curious fact. She is rather deaf on the left side, and when young was very deaf of both ears, but much worse on the left side. Her pupils are equal, and her sight good in both eyes. She is very hysterical, and suffers often from globus hystericus, and from a sensation of heat and weight on the top of her head and palpitation on exertion or excitement. Her urine varies greatly in quantity, sometimes being scanty, at other times very abundant. Her bowels are generally confined, and she is troubled with a cankery taste in the morning. Her menses are very scanty and irregular, intermitting sometimes for six months. She does not blush more on one side of the face than the other; and when excited her ears become both equally very red. The skin of the face presents the same aspect on each side. The radial pulses appear to beat in all respects equally. Five months ago she was confined, and since then all her troubles have much increased. The left breast yields much less milk than the right, this being full and distended, while the left is flat and empty. Shortly before her visit to the hospital she was seized with neuralgia of the auriculo-temporal branch of the inferior division of the fifth nerve on the left side, the paroxysm being accompanied by salivation of the left side of the mouth; but previous to this neuralgic attack the secretion of saliva was no greater on one side of the mouth than on the other. During an attack of pain the left side of the face sweated greatly.

She has lost two children—one from measles, the other from hemiplegia. Deafness is a family complaint; no other member of her family suffers from nervous disease, and none are affected with unilateral sweating. At fourteen years of age she suffered for a year with twitchings of the right side, the arm being especially affected, but this does not appear to have been true chorea; and still on excitement there is a disposition to involuntary movements of the right arm.

The application of a weak belladonna-ointment to the left side of the face for five to ten minutes, three times daily, greatly reduced the sweating and equalized that of the two sides. It, moreover, cured the neuralgia.

The author has met with cases of profuse local sweating over the loins, covering a surface rather larger than the hand, and exciting a copious eruption of eczema. Here the belladonna-liniment checked the sweating, and the eczema at once disappeared.

In some cases of sweating the belladonna no doubt fails. Thus in one inveterate instance, so far from affording relief, it increased the sufferings and sweating in the case of a man who for twenty-two years had been affected with an occasional eruption of the hands and feet, looking at times like eczema, but at other times putting on the appearance of lichen. In cold weather he is pretty well, unless he takes exercise or sits in a hot room; but in March, as the weather begins to get warm, the symptoms set in. His hands and feet swell, and feel so tight as if they must burst. At the same time he sweats a good deal, but not while lying down; but directly he rises, or even sits, the sweating begins. The sweating is much more abundant in his hands and feet, especially at the finger-tips and the thumb-balls; and at the tips of the ring and little fingers of the left hand it is especially marked. The sweat runs down in drops from the hands, and when he wipes the finger-tips he can see the sweat oozing from the pores of the skin. The outer part of his hands corresponding to the fifth metacarpal bone is also the seat of great sweating. At the tips of his fingers and the thumb-balls he suffers from severe pricking pain, which he likens to little insects biting their way out. He feels hot all over, and calls it heat in the blood. The attacks are accompanied by a good deal of itching over his back. The skin of the hands about the nails becomes hard, cracks, and bleeds. A little rash similar to that described appears in the clefts of the fingers and over the back of the hand between the thumb and forefinger. In this patient's case, as has been stated, belladonna-ointment applied to the hands greatly aggravated all his troubles and increased the sweating.

In order to test still further the effects of belladonna on sweating, many experiments of the following kind were performed by Mr. C. A. Nankivell on several occasions: A patient in University College Hospital, after undergoing a sweating in the hot-air bath, was rubbed with belladonna-ointment on one side of the face for ten minutes, three times a day, for two or three days; then the bath was repeated of the same temperature and duration, when it was observed that the sweating both during and subsequent to the bath was very greatly lessened, and that the effect was general, although the ointment was applied only to one side of the face. On some occasions the ointment was rubbed into the chest, but then the effects were much less marked than when applied to the face, possibly because less of the ointment was absorbed.

As the local application checked sweating over the whole body, it was concluded that it acted by its absorption, and this led to the internal administration of belladonna, but its repressing effect was apparently decidedly less than when locally applied, possibly because less of the drug was given by the mouth than was absorbed by the skin. Still, no doubt the internal administration of belladonna does sometimes effectually control sweating, as the author has often witnessed in the case of weakly children perspiring profusely on exertion or while sleeping; and in the following curious case of a middle-aged man who, after much mental worry, suffered from excessive sweating of both cheeks while eating, especially hot meat or vinegar, the sweating ceased immediately after the meal. This man passed at times a profuse quantity of pale urine. Ten drops of tincture of belladonna taken three times a day completely checked the sweating.

Since writing the foregoing, the author has made some further experiments with very striking results. A middle-aged man, a sufferer for several years from unilateral sweating of the right side of the face and neck, applied for relief from a pain in his side. He was found to be sweating very profusely from the right side of his face and neck, from exertion and the great heat of the weather. To relieve his pain, a twelfth of a grain of morphia was subcutaneously injected, which appeared to increase the sweating, although it was difficult to be sure of this: while still sweating profusely, so that on wiping his face the sweat could be seen rapidly oozing from the skin, we injected $\frac{1}{100}$ of a grain of atropia under the skin of his arm, and in about a minute the sweating entirely ceased, and his face remained quite dry, till his dismissal about three-quarters of an hour after the experiment.

To a middle-aged woman suffering from acute rheumatism, a hot-air bath was administered, followed by cold sponging. This treatment caused her to sweat so freely that, for several hours after, the perspiration continued to pour down her face, soaking her clothes and the bed-linen. While in this state, $\frac{1}{100}$ of a grain of atropia was subcutaneously injected into her arm, and in about a minute the perspiration ceased, and for two hours her skin continued dry and she felt much cooler, but in the evening rather free perspiration returned. We next gave a young man a Turkish bath, and Mr. Johnson, the resident assistant of wards, who has helped me in these observations, joined him in the hot chamber. Both sweated freely, and then each was injected with $\frac{1}{100}$ of a grain of atropia, and in a little more than a minute the skin became dry, and the perspiration did not return after the application of the cold douche, nor afterward. Mr. Johnson remarked that, so dry did his skin seem, that he felt he should never sweat again. They both suffered from much dryness of the mouth, but their pupils were not dilated. We next placed a boy in the hot-air bath, the temperature rising to 180° Fahr., and when sweating freely we injected $\frac{1}{150}$ of a grain of atropia, and almost immediately the sweating ceased and did not return.

2.—*On Hæmophilia.* By J. West Walker, M. B., London. [British Medical Journal, June 8, 1872.]

The following family history is interesting, as furnishing examples of hæmophilia, a disease which has recently been brought prominently before the profession in this country by Dr. Wickham Legg. I intimately knew the father and mother anterior to their marriage eleven years ago; and both before and since that event have been their constant medical attendant. I fail to trace any facts bearing upon the important question of hereditary character, and am inclined to think the present an instance illustrative of the first start of a pathological condition which, there is every reason to fear, will be transmitted to descendants. The ultimate cause of the phenomena is involved in obscurity. As, on the one hand, some, so to speak, unhealthy selection seems to bring about the development of this singular morbid process—so, on the other, a more healthy selection, through more or fewer intermarriages, tends to affect its diminution and ultimate eradication. Hence, notwithstanding the hereditary tendency is a most important and diagnostic feature, we often fail, happily, in our endeavors to trace it in full force through more than a limited number of generations.

The father is a clergyman of the Church of England, and master of a grammar-school. He is very tall—six feet four inches—and thin. He is a hard-working and somewhat delicate-looking man, and carries with him the appearance of worry and anxiety. He has never since I have known him had any serious illness, but he occasionally suffers from headaches, which find relief in epistaxis. His age at marriage was thirty-nine. He is no relation to his wife, and he never heard of a bleeder in his family. His father, a clergyman, died at the age of sixty-two, of heart-disease; his mother at the age of seventy-four, of general decay. She was a remarkably healthy, active woman up to seventy, when she experienced an attack of hæmorrhage (hæmatemesis?), which left her weakly and with health impaired during the last four years of her life. He has three brothers, all married, two with children (no bleeder), and two sisters unmarried.

The mother is a perfect model of a healthy woman. She is fair, stout, and somewhat short—a contrast to her husband. She married eleven years ago at the age of twenty-six, and has since borne seven children and had two abortions. Her labors have been natural; and neither at those times nor at menstruation has hæmorrhage been excessive. She is an excellent nurser. She has never had any serious illness, and always lived in the midst of good sanitary surroundings. She knows of no bleeder among her relations. Her mother, living, and aged seventy-two, has been twice married, and had by her first husband (1) twins, a girl and boy, both of whom died within the month; (2) a boy, who died in infancy; (3) a boy, who is now living, aged forty-eight, with six children—no bleeder. By her second husband she had three girls: the first died young; the second is the lady in question; the third is living, aged thirty-four, unmarried. Her father is living, aged sixty-seven, and has always enjoyed good health.

The children of this marriage are delicate-looking, though intelligent and lively. They were all born at the full time and in a healthy locality. They are as follows:

1. A girl, aged ten, born December 17, 1861, not a bleeder.
2. A boy, Charles, aged nine, born April 6, 1863, a bleeder.
3. A boy, Edward, aged seven, born August 19, 1864, a bleeder.
4. A girl, aged six, born January 24, 1866, not a bleeder.
5. A boy, George, born June 15, 1868, a bleeder, lived sixteen months, and died after scarlet fever.
6. A girl, aged two, born November 5, 1869, not a bleeder.
7. A boy, Gerald, aged one, born February, 1871, a bleeder.

Charles first showed bruises when two months old, and has from then to the present time constantly presented them. The bruises occur on any part of the tegumentary surface, and are often large, extending, in some instances, from shoulder to elbow, or from hip to knee. They are very painful, the pain being paroxysmal, with violent nightly exacerbations. The child will lose blood in this manner in quantity sufficient to produce great weakness, and to blanch the skin and mucous membrane. Frequently the bruises can be traced to injury, often, however, very trivial; in some instances they seem to be spontaneous. In 1864, at the age of sixteen months, the first attack of external bleeding took place. It commenced from the nose, but during the same week he fell, injured the upper gum, and bleeding ensued. The nose-bleeding soon ceased, but that from the mouth continued a fortnight, bringing the child to the verge of death. The bleeding came to an end, as it were, spontaneously; a rapid recovery followed. In 1865, bleeding again started from the gums after injury. This attack, not so violent as some, lasted probably a week, but was succeeded after an interval of two months by a fresh outbreak, which continued about the same length of time. After this he remained free until the following January (1866), when he bled for three days from a wound of the upper lip. He continued to have bruises, but had no serious external hæmorrhage until 1869, when he took scarlet fever. In the course of the complaint he was delirious, and had two attacks of epistaxis. Convalescence was tardy and critical, protracted by glandular inflammations, abscesses, and aphthous conditions. During this period he bled twice from the mouth, the hæmorrhage lasting upon each occasion ten days, and producing extreme debility. He, however, struggled through all difficulties, and after a time regained health. After this, he remained free from bruising or bleeding for twelve months, until one evening in 1870, when just before going to bed he cut a finger. The servant tied up the wound, and, fearing disgrace, put the child to bed without acquainting her mistress. Throughout the night the poor little fellow bled through dressing, bed, and bedding, even on to the floor. In 1871, he bled once from a bite of the tongue; it was comparatively easily stopped. Altogether the boy is healthier, gains flesh, bears fatigue better, and bleeds seldomer. He has been vaccinated, and had measles in a satisfactory manner. His joints have never been specially affected.

Edward first showed bruises when about a month old, and has had them from time to time ever since. At the same time as his brother (1869) he took scarlet fever. He got over the attack satisfactorily upon the whole, but has since then manifested the peculiar and characteristic symptom of hæmophilia—joint-swelling. This complication is excessively painful. It affects the larger joints, knees, and ankles, but shows no preference for either side. It resembles in many particulars an attack of bruising; and I incline to think that the symptomatic differences are to be explained by reference to the structures in which they occur; being fibrous rather than cellular, pain is increased, and serum rather than blood effused. He has bled externally upon three occasions, always from the tongue—(1) from the side, (2) from beneath, (3) from the dorsum. The last hæmorrhage happened in November, 1871, and was very profuse, continuing fourteen days, producing great prostration, and making the child as white as marble. He, too, has been vaccinated and has had measles.

George early manifested the tendency to bruise, but not quite to the same extent as his brothers. He, too, was seized with scarlet fever, and passed favorably through the primary disease. It left him weakly, and he became a prey to sequelæ. Among other drawbacks, a submaxillary gland inflamed, suppurated, and, although the abscess was left to open naturally, the internal surface of the sac exuded blood which, in spite of all attempts to restrain it, welled up through the fistulous opening. The child was cutting his teeth at the same time, and blood seemed to flow, as it were, from all parts of the gingival mucous membrane. He gradually sank, aged sixteen months. For two days before death, the surface of the body was thickly covered with petechiæ, and one large ecchymosis presented itself over the left eye.

Gerald, an infant fifteen months old, is an average-looking child, but has shown a marked tendency to bruise, beginning before he was ten days old. As yet he has not bled externally. He has cut five teeth.

The three girls are entirely free from this singular affection. They never bruise, and wounds and injuries heal readily. I have on two occasions extracted a molar tooth from the eldest, and experienced no trouble from subsequent bleeding.

Post-mortem examinations have hitherto thrown no light upon the nature of the affection; nothing more than the usual appearances of death after hæmorrhage has been revealed; consequently its pathology is as yet but imperfectly made out. There is, however, a sufficient assemblage of facts to show that we have to deal with a condition different in many respects from what has hitherto been comprehended under the term "hæmorrhagic diathesis," and worthy of a name and place in our nomenclature of diseases. The hereditary character, the number of children in a family, the selection and limitation as to sex, the extent, painfulness, and frequent repetition of the sanguineous effusion, the peculiar affection of the joints, the (what for want of a better term must be called) eliminating character of the morbid process, and, lastly, the influence of drugs, collectively point to the diagnosis of a disorder unique, and to be distinguished from any of the affections known in our nosology, or described in our English textbooks.

In the treatment of these abnormal developmental conditions, attention to hygiene becomes of the first importance. Healthy habitation and habit, suitable food and clothing, effect more than drugs. For these unfortunate children it becomes necessary, though hard, to forbid the more violent of boyhood's games. Whether the bleeding be subcutaneous or external, rest, mechanical and physiological, becomes essential. The treatment of the disease is at best unsatisfactory, but this is a matter which becomes robbed of much of its disappointment when one has observed the natural history of the complaint, and how its tendency is, after a certain time, after, as it were, a certain amount of blood has been drained off, always toward recovery. Most of the usual remedies in purpura and other hæmorrhagic diseases have failed in the treatment of my cases, thus furnishing an additional reason for believing the affection to be in its nature different from its apparent allies. For the bruises, contrary to custom, I avoid cold. Evaporating lotions and arnica do no good. Warm sedative fomentations and poultices alone give relief to pain. Even after the acute stage is over, stimulating embrocations avail little or nothing, for I have always observed that, if left alone, the effused blood rapidly disappears under food and exercise. The external bleedings in my cases have caused increased trouble and anxiety by occurring, for the most part, in the cavity of the mouth. The necessity for taking nourishment, the constant flow of saliva, and the tendency to be ever cleaning the part with the tongue, render all dressing difficult and insufficient in this situation. From the mouth, too, much blood is swallowed, thus interfering with appetite, nauseating, and bringing on vomiting and diarrhœa. I have lost all faith in external styptics—none, in my hands, doing the least good. I have given a fair trial to solid nitrate of silver, copper, lead, alum, chloralum, perchloride and pernitrate of iron, matico, ice, etc. Pressure when practicable is beneficial, but, owing to the weak coagulability of the blood, it must be maintained many days. Reasoning from the proved good effects of iron in hæmorrhagic and anæmic affections, we are tempted to resort to it with confidence in the treatment of ailments presenting these two marked symptoms; but, according to my experience, only to be disappointed. I have given the wine and tincture freely as a preventive and a curative, and during convalescence, but can speak of no results. I used to think that convalescence was hastened by the tonic, but later observation of the rapid restoration to health of cases left to themselves has shaken my confidence on this head. I have also given internally the mineral acids, gallic acid, lead, and alum, but have no praise to bestow. Turpentine is the only drug which has uniformly given satisfaction; and so much is this the case, that now, whenever bruises are more than usual, it is our constant practice to resort to it. The mother beats up one drachm of the medicine together with the yolks of two eggs; and of this mixaure one-sixth or one-eighth part is given two or three times a day, until the system is brought under the influence of the remedy. This treatment may be undergone many times during the year; and I firmly believe that it has the power of warding off many bruises and much external bleeding.

Attention to diet becomes important in the management of these cases. Fresh meat, or a good extract thereof, pure milk, and eggs, should be partaken of freely; ordinary vegetables in moderation; ripe fruit somewhat in excess. Among beverages, wine is useful during convalescence, and ale and porter as articles of diet. I confess a fondness for spruce-beer, which probably contains some of the terebinthinate constituents of the fir, and thus combines, in a pleasant form, food and medicine.

In this short and necessarily imperfect history much has been omitted, and I have forborne to enlarge upon the trials and troubles which the constant attendance upon such cases entails upon doctor and parent. I have ventured to publish it, partly as a slight contribution to a hitherto overlooked subject, and partly for the sake of giving comfort and encouragement to any to whose lot it may fall to have to contend with the troublesome affection.

3.—*Diphtheritic Albuminuria.* By R. Browning, L. R. C. P. L., etc. [British Medical Journal, July 27, 1872.]

The pathognomonic importance of albuminuria when occurring in the progress of true diphtheria is a practical point which, strangely enough, I find, after some painstaking, either totally passed over, or at least barely mentioned in the writings and *viva-voce* cliniques of the highest authorities I have yet been able to consult.

In the absence of *post-mortem* observations of my own, it is with some diffidence that I venture to put before you what I have reason to consider an equally novel and successful treatment of this momentous symptom. I would premise that by the term "true diphtheria" I wish to specify that dangerous disease known, unfortunately, but too well to most of us in this neighborhood, characterized especially by the appearance of a false membrane on the surface of the pharynx, which membrane can be removed in strips by a forceps, and consists of numerous epithelial scales and cells, with some occasional pus-globules, all interspersed in the intestines of fibrinous filaments, to the exclusion of so-called "genuine croup," of "laryngitis stridulosa," and of any bad forms of tonsillitis, muquet, or local scarlatinal lesion. [I have placed "genuine croup" in this *index expurgatorius* from deference to the opinion of most English authors; but many years since I was taught by Dr. Semple, at the St. Pancras Dispensary, that it was really and actually the diphtheritis of Bretonneau, and the same is held by most modern French writers.]

From what I have lately witnessed while watching two local epidemics of diphtheria, I am disposed to consider—

a. That albuminuria is present in nearly all cases.

b. That its appearance is usually about the end of the first week after the diphtheritic membrane is developed, though sometimes earlier, more rarely later.

c. That, coexistently with its appearance, a notable diminution of the quantity of the urine and an increased excretion of urea occur (the latter sometimes equalling treble its normal amount); while lithates (with uric acid, oxalates, and sometimes phosphates) generally, tube-casts, both granular and waxy, frequently, blood-corpuscles not seldom, and pus-globules occasionally, are found on examination of what is secreted. The urinary specific gravity mostly averages 1016, and the temperature of the body is, as a rule, from 100.4 to 102 degrees.

d. That the gravity of the prognosis increases in an equal ratio with the quantity of albumen existing in the urine, independently of the amount of throat-affection or kidney disorganization, and that an early or late discovery of albumen is of serious import.

e. That local mischief attacking the pharynx, larynx, or other structures, and paralysis subsequently occurring, are entirely the result and symptomatic of a morbid poison affecting the general system, just as the sore-throat of syphilis is the sequence of a blood-disease previously contracted.

f. That albuminuria, in any quantity, is due to obstruction of circulation through the kidneys, caused by congestion of the Malpighian tufts, this congestion being produced by paralysis of the nerves supplied to them; but that a mere trace only arises, either from pus or else blood-corpuscles, which have casually entered the volume of urine.

g. That the indication of treatment, in albuminuria occurring to an appreciable degree during the progress of a case of diphtheria, is to remove this obstruction by overcoming the paralysis, and that local Faradization affords the best method of doing so.

h. That, after albumen has disappeared from the urine, it may return, synchronously with a relapse of diphtheria; and that throughout the whole course of this malady little, if any help, is derived from the use of the clinical thermometer.

[Details of seven cases, tending to support the above views, were here read.]

You will observe that *both* the cases which terminated fatally were those in which no Faradization was employed, and that *five* others, all of very serious nature, recovered after it was resorted to; *all* were marked by unmistakable evidence of blood-poisoning, and albuminuria, with more or less suppression of urine, was noticed in each. The treatment of all was conducted on the same principle, *plus* or *minus* the induction-coil; the object aimed at being at first, during the premonitory symptoms, to regulate the secretions, and then to support the strength of the system in every possible way. My sheet-anchor was the tincture of perchloride of iron sometimes combined with glycerine, sometimes with chlorate of potash, and sometimes given *per se*. Stimulants and nourishment in every variety were supplied with no sparing hand. Iodide of potassium proved a disappointment. The customary topical medication was, of course, attended to. In some instances, the ordinary conductors fitted to most galvanic batteries; in others, "etnas" of this construction were employed ("etnas" shown). Faradism was thus employed over the lumbar regions along the lower part of the spine, and as nearly as possible in the direction of the ureters. Its use in this manner is, I think, an entire novelty.

I must confess that I am thoroughly at a loss to determine the special cause—the *fons et origo* of diphtheria. We know that the blood is charged with urea as soon as, if not before, paralysis of the kidney occurs; but so it is in other diseases, and yet a totally different train of symptoms is witnessed. I look forward to being enlightened on this point by more able and experienced observers.

4.—*A New Theory of Gout.* By Austin Meldon, M. D. [Lancet, July 27, 1872.]

A glance at the history of gout will convince the most incredulous that little progress has been made in the pathology of the disease. There is, moreover, some reason to believe that in its treatment we are not much in advance of the ancients. The humoral theory of Hippocrates and Galen does not differ essentially from the uric-acid and urate-of-soda doctrine of the present day; and there is much uncertainty as to whether our forefathers did not know of the existence of colchicum or some kindred plant. Be this as it may, one thing is certain—gout is now a more common and a more fatal disease than heretofore. Within the last twenty years, at least, six thousand persons have died in the United Kingdom directly from this disease, and ten times that number have perished indirectly from it. I, therefore, deem no apology necessary for laying my views of the nature and treatment of this important affection before my professional brethren, believing it to be the duty of every medical man to forward as far as he can the pathology of the disease.

Cullen, who believed gout to be an affection of the nervous system, had few followers; it was difficult to explain by this theory many of its phenomena, and it soon, therefore, passed into oblivion. It was replaced by the reappearance of the humoral theory, which lasted until Dr. Garrod's experiments proved the existence of uric acid in the blood of the gouty. The late Dr. Gairdner believed that the disease was due to venous congestion, but, with the exception of a few of his followers, here and in Germany, most medical men have adopted the uric-acid theory.

True, many do not believe, with Dr. Garrod, that the kidney is the organ whose functional impairment causes this undue accumulation of uric acid, but refer it to derangement of the stomach, liver, or kidneys; the principle of the doctrine is, however, universally received. I have, nevertheless, little doubt that every practical physician will agree with me, that the presence of uric acid or urate of soda cannot be the sole cause of this distressing malady. In Ireland, gout is one of the rarest affections met with in hospital practice, yet I have repeatedly found the blood of otherwise healthy men, lying in our accident-ward, loaded with urates; and my own blood has been in this condition for years, although I have never had the least symptom of gout. Dr. Gairdner found urates in the blood of a boy four years of age, in whose family gout had never been known. It is, therefore, necessary to find another link to complete the chain; this, I think, is furnished by Cullen's theory.

The predisposing cause of gout is undoubtedly the presence in the blood of uric acid, and of soda in some form; nerve-force, I believe, when in a healthy condition, preserves these two in a fluid state, separately, in a condition in which they may be eliminated by the skin, kidneys, or bowels. As soon, however, as this nerve-influence is lessened, these two substances unite in the tissues most removed from the brain and centre of circulation. Irritation and inflammation excite the nervous system to increased energy, and the disease for the time is arrested; often, however, a like cause produces a second exacerbation, a third, or even a fourth, and then Nature gradually alters or removes that which has been deposited, and all traces of the fit have passed away. The time that an attack generally commences is at night, when both nerve-force and the circulation are weakest; the position, moreover, most usually affected—the great-toe—favors my theory; and I need scarcely mention the numerous instances recorded where a fit of gout has been brought on by great nervous depression. Columbus never suffered from the disease until disappointment and ingratitude depressed his whole nervous system. Hundreds of similar cases are familiar to all; politicians and speculators are particularly liable to gout.

The action of colchicum furnishes me with one more proof. By an experiment which may be repeated by any one at will, I have satisfied myself that it is a nervous stimulant. Repeatedly have I, while fasting and in perfect health, taken doses of from ten to fifteen minims of tincture of the seeds of colchicum. Its effect was to produce, first nausea, and then increased action of all the organs of the body; the skin became moist, the action of the kidneys and liver increased, and the mental faculties were invigorated. On some occasions the heart's action was much increased, and I have even experienced severe palpitation consequent on its use. All these results can only be produced by a nervous stimulant. This is the only way, too, in which its apparently-magical influence on a seizure of gout can be explained, and it accounts also for the injury which its too-frequent use may produce.

In conclusion, I have to recommend for trial, in the treatment of chronic gout, some medicines from which I have, in my practice, derived the greatest assistance.

Sulphate of nickel and the triple phosphate of iron, quinine, and strychnia will be found of the greatest benefit.

There is but one other point connected with this subject which I desire to mention. It has often been doubted that the skin can eliminate uric acid; but, if a large quantity of perspiration be collected, and, after evaporation, tested, abundant crystals will be obtained. For this experiment it is necessary, in the first instance, to have a large quantity; I have repeatedly obtained as much as a pint, by aid of the Turkish bath. From a small amount of perspiration I rarely succeed in obtaining positive proofs of its existence.

Miscellaneous and Scientific Notes.

Clinical Remarks on Peritoneal Tuberculosis. [Prag. Viertljsch.] By KANLICH.—Only those rare cases of tuberculosis peritones are here considered where the tuberculosis occurs in the peritonæum exclusively as primary, or at least as the predominant form of disease, while a long period of the course of the affection occurs without other organs being drawn into diseased participation.

Aside from the exceptional cases, there generally occurs a successive progression of the affection from a fixed point; there is also, as a general thing, with the appearance of the neoplasm, ascites, and also vegetation of fresh conjunctive tissue, which may cause a gluing together of the layers of the peritonæum, and of the intestinal folds. With the usually prolonged course of peritoneal tuberculosis, there are found changes in the tuberculous masses which must be "regarded decidedly as an anatomic involution, an anatomical process toward cure." We find, namely, at times, with the absence of any fresh peritoneal irritation and of any exudation, the tuberculous masses hard and dry; others, especially large conglomerates, degenerated into a cheesy soup, or a death-like mass, or entire plates of conglomerate, in such a state of entire fatty degeneration that Virchow's idea of the healing of tubercle by resolution appears very plausible.

Most frequently with tuberculosis of the peritonæum are associated tubercles in the pleura (usually the left) of the lung. Further, the kinking of the intestinal folds, the tendency of the manifold pseudo-membranes to bleeding, and, as before remarked, the regressive changes of the neoplasm, deserve especially notice in every case. The etiological causes generally coincide with that of the tuberculosis; as to age (children were not admitted to the institution in which these observations were made), Kanlich's youngest individual stands at sixteen years; the majority of the cases lie between the thirtieth and fiftieth years; in single and rare cases there was neither previous evidence of scrofulous affections nor the evidences of tubercles elsewhere, nor inflammatory processes ending in cheesy formation. In the majority of the cases, nevertheless, one or the other was the case. To the influence of cold as an etiological cause, "of course no importance was attached."

Symptoms.—The fever-symptoms at the commencement of the disease are generally very slight; exceptionally there occur at once a marked rigor, great heat, and great debility. In one of the author's cases there occurred at once startling rigors and consequent collapse. In the further course, occur nightly exacerbations of usually moderate intensity, and night-sweat; still there are remissions of several days or even months. In regard to the tenderness (pain), the greatest differences present themselves: pain is at one time very slight; again, although rarely, at the very outset, very severe and extended, but has nothing characteristic. Very variable is the condition of the exudation; there occurs with the development of the tuberculosis at times a slight, at other very considerable, peritoneal effusion. In single rare cases no full effusion whatever occurs during the entire course of the disease, but, instead of this, merely band-like thickening of the strongly-retracted abdominal walls. Also an existing effusion may be subjected to great variations; it may be either entirely resorbed and then those phenomena may become marked which depend on the neoplasm itself, and especially on the false membrane; variously-shaped bosses and tumors become evident, sometimes placed transversely, corresponding to the shrivelled, thickened net-work; again, the intestinal folds form several convolutions contracted against the vertebræ and separated by deep depressions, which are covered with thick and appear like large, rounded tumors. As of rare occurrence, it deserves mention that a largely-developed pseudo-membrane, by adhesion to the abdominal walls, may effect a complete separation of the cavity, so that, at the same time, a decided *hydrops saccatus*, and, besides this, a free effusion into the abdominal cavity, may be present.

A constant symptom from the commencement of the disease until (through the mechanical relations of the pseudomembrane) even after the involution of the local affection, is a greater or less degree of meteorism; this is probably mostly owing to the implication of the muscular layer of the intestines; if adhesion of the intestines and shrivelling of the mesentery have occurred, it is no longer, as before, the upper portion of the abdomen which is pressed forward by the crowded intestines; by repeated observations there is found here and there a changing reverberation of the otherwise dull, tympanitic percussion-sound. As well through the pressure of the fluid in the peritoneal sac, as from the contracted processes, derangements of circulation occur, and consequently œdema of the lower extremities. The spleen is generally enlarged, either from acute tumefaction or through amyloid degeneration. As to the condition of the kidneys, the author repeatedly observed a lethal termination after a sudden copious elimination of a clear, water-like urine, with cool extremities, and imperceptible pulse. Kanlich assumes three differerent forms of the clinical course of peritoneal tuberculosis: 1. The disease begins suddenly with marked fever-phenomena, circumscribed pain at one spot, or over a greater extent of the abdomen, and repeated severe vomiting meteorism. Stypsis, on examination, shows the objective signs of a circumscribed peritonitis (pain on pressure, slight distention of the belly, increased resistance and dulness); after a shorter or longer improvement of all these symptoms the same scene recurs, and the process extends with intermissions over the whole belly. It finally comes to a complete picture of peritoneal tuberculosis, with all its signs as above. The entire process lasts several weeks, the patient growing very thin meanwhile; after a longer pause, in which only the consequences of intestinal adhesions show themselves (stypsis, vomiting), and in which the patient continues to grow meagre, the case ends with a final pleuritis. At the same time, caused by the local processes, there occurs a copious intestinal hæmorrhage, or a secondary peritonitis through perforation.

2. A second series is formed by those cases which generally come under treatment with phenomena of ascitis, with or without œdema of the lower extremities. Preceding the development of the ascites, occur slight pains in the abdomen, a feeling of pressure and tension, occasional vomiting and stypsis, loss of appetite, and slight fever-movement. Besides the other signs of ascites, meteorism and retarded movement of the intestines. After a long continuance of this condition, it proceeds, with a marked increase of the fever, to an effusion into the pleural cavity and the pericardium, and this consequence of the similar affection of several serous membranes assures the diagnosis. The picture of the disease may be changed by the fact that the exudation into the peritoneal sac is combined with hæmorrhage, which is recognized by a sudden paleness, cold extremities, very frequent, small pulse, faintness, etc.

3. The cases included in this group are marked by a "decided involution of the process." After long continuance of the disease as circumscribed to the peritonæum, the fever-symptoms are wanting entirely; with a decrease of the hydrops and increased diuresis, the conditions of assimilation improve; there remain behind, perhaps, only slight meteorism and certain disorders in the function of the intestinal canal produced by the anatomical lesions; or the peritoneal contractions are evident to the touch. After a long standstill, perhaps for months, even in these cases there follows tuberculosis of other serous membranes, of the pericardium, the pleura, or acute miliary tuberculosis, and a fatal termination is almost inevitable. The diagnosis is beset with many difficulties, since, as we have seen, there is no characteristic symptom. The exclusive limitation of the hydrops to the lower half of the body, and the absence of cyanosis, lead to the exclusion of heart or kidney-disease. If the lungs, apart from the compression by the ascites, are healthy, and the heart, apart from its high position, sound, and the urine normal, we have less difficulty in the diagnosis.

Bacteria and their Relations to Putrefaction and Contagion.—(Rundsc. Med. Wiss., 1872, No. 12.) Cohn has instituted a comparison between the decomposition of nitrogenous and non-nitrogenous substances. The decomposition of the former is fermentation; that of the latter is putrefaction. Putrefaction receives especial consideration, and he comes to the following results: Bacteria are the constant accompaniments of putrefaction. If their access to nitrogenous substance (albumen) be prevented, no putrefaction can occur. On their increase depends the grade of putrefaction. After the completion of putrefaction, no further increase of bacteria is demonstrable; they lie as a deposit of gelatinous lumps (zooglea) or as a powdery residue at the bottom, like yeast, after the completion of saccharine fermentation. The bacteria introduce the putrefactive process, as excitants of decay (soprogena), while other minute organisms (mould, fungi, infusoria) are merely accompaniments of decay (saprophile). No genealogical connection between fungi and bacteria is demonstrable.

The advent of bacteria to substances passing into decay occurs through the agency of water, or through the vascular walls (according to Burton Sanderson) in which the substance is contained. From the air, fungi only are introduced, or, in sufficient numbers, bacteria, in albuminous bodies, so that urine, saliva, blood, pus, and milk, exposed to the air will mould but not putrefy. If, however, these bodies are brought in contact with water containing bacteria, or with the walls of vessels containing the same, putrefaction immediately sets in. The nitrogen of the protoplasm of the bacteria is not drawn from the nitrogenous material of the substance undergoing decay, since the bacteria, like green plants, obtain the nitrogen of nutrition from ammonia or nitric acid, and then assimilate it. Sanderson has shown by experiments the Pasteur's solution (100 pts. water, 10 pts. sugar-crystals, 1 pt. tartrate of ammonia) to be a good generating mixture for the nourishment of fungi and also for bacteria. The experiments of Prof. Cohn go to show that for bacteria sugar is not necessary.

Bacteria increase in every fluid which contains, besides NH^3 or NO^3H, any non-nitrogenous carbonaceous substances. It follows from what has been said that the active effect of bacteria is its reduction of albuminous combinations into NH, which becomes assimilated and splits into a series of sub-products, like ferment fungi in sugar fermentation, into alcohol and a series of sub-products, at times colored sub-products (purple-red masses in potatoes, bread, etc., monas-prodigosa). The excitants of this pigment putrefaction are not staff or cylinder bacteria (bacterium termo), but the so-called spherical bacteria in which the innate movement is absent. Prof. Cohn has succeeded in producing pigment putrefaction in solutions of acetate of ammonia and tartrate of potash. The color of this solution was finally blue, and changes in like manner to tincture of litmus through the action of acids and alkalies. In conclusion, the author refers to experiments which resulted in the finding of bacteria in the blood of various patients. He is in position to confirm these; but found in the blood of patients with contagious diseases only spherical bacteria. These, according to the author's opinion, act injuriously through the side-products, which are produced in consequence of a decomposition introduced into the blood of the organism. Finally, drinking-water is regarded as the principal carrier of bacteria into the organism.

Wohlrab on the Evidences of Degeneration and Hereditary Neuropathies. (Arch. f. Heilk., 1871, 4, 5.)—It is well known that in children of parents with mental or nervous diseases, disorders of the nervous system occur, sometimes similar, often, however, of an entirely different kind, and with greater intensity; it is here the question of a degeneration of the race, which is also often signalized by congenital departures from the formation of certain organs, even not belonging to the nervous system frequently, or by the presence of such signs of degeneration. To Greisinger is due the merit of having most expressly and repeatedly called attention to the value which these diagnostic signs possess as accompaniments of a large sum of hereditary psycho- and neuropathies.

As the physician in private practice has frequent opportunity of meeting such signs of degeneration, it may not be without benefit to reproduce them in the series as given by Wohlrab:

1. Deformities of the ear: narrow ear, narrow helix, extension of the upper skin of the ear-lap, without a break or with only slight indentation of the skin of the cheek; frequently rudimentary ear-laps.
2. Of the eye: coloboma of the lids, of the iris, abnormal size or congenital atrophy of the bulb; platymorphia and enthymorphia of the bulb, asymmetry of the cornea and the lens, excessive myopia, different coloration of the two irides.
3. Smallness of the body.
4. Of the genitals: fissured sexual development, abnormalities of the genitals (errors of development of the penis, defects of the uterus), sterility.
5. Defective formation of the teeth: as, abnormal placement of the teeth—twisting on their long axis, abnormalities of size, dark transverse lines on the incisors.
6. One-sided action of the N. facialis.
7. Unfavorable development of face, among which hare-lip is to be included.

8. Abnormal formation of the skull.

9. Diabetes.

10. Of the organs of speech: stuttering, too high or too low a pitch of voice.

11. Of the external skin: nævi, telangiectasis.

12. Arrest of development in the members: defects of the fingers and toes, absence of a part or the entire limb, etc.

13. Also abnormalities of the internal organs certainly belong here, but the author is not able to give any observations of his own. Such signs of degeneration are rarely met with singly; they are generally several; most numerous and most marked occur in idiots. These signs of degeneration point to the hereditary affection of the individual and his blood-relations, but are not by themselves a measure of the neuropathic infection; for this only the sum of the psychical, motor, sensitive, and trophic neuropathies can avail. That, in spite of the large sum of symptoms already mentioned, the betrayal of possible signs of degeneration is not exhausted, is evident when we consider that, for example, of the anomalies of the growth of the hair, rachitis, curvature of the bones, goitre, etc. (which, much more frequently than many of the signs already mentioned, may accompany degeneration, or, in the author's words, point to an hereditary influence), no notice is taken.

The Role of the Nerves in Artificial Production of Diabetes. E. Cyon & Aladoff. Bullet. de l'Acad. Imp. de Petersbourg, viii., 91–109. (Centralblatt f. d. Med. Wissenschft., 1872, 10.)—From the experiments of others and their own, the authors conclude that diabetes may be caused by a paralysis of certain nerve-fibres which are in connection with the first thoracic and last cervical ganglia. They then examined the nerves which enter and leave the inferior cervical ganglion, and found that diabetes occurs only when the two vertebral branches are divided, or else the two nerves leading to the ganglion stellatum, which surround the subclavian artery and form the so-called annulus Vieussenii, while division of the other nerves has no influence in the production of sugar.

To follow still further these nerves, the principal trunk was divided subcutaneously between the tenth and twelfth ribs. After this operation diabetes was almost never produced in dogs; but also, after subsequent division of the ganglion stellatum and inferior cervical of the same side, there was no effect. If, however, extirpation of the ganglia had excited diabetes, the subsequent division of the nerve did not remove it.

These observations, which correspond with Bernard's, lead to the conclusion that in the splanchnics are two kinds of fibres, one from the ganglion stellatum, which by paralysis leads to diabetes, the other from the cord, paralysis of which hinders the product of diabetes.

Next was to be noticed the supposition of Schiff, that in these cases was to found the effect of vaso-motor influence. The authors noticed that, on irritating the ring of Vieussens, the surface of the liver showed contraction of the smaller vessels, while white spots appeared corresponding with the boundaries of the acini. A T-canula was put into the hepatic artery and the pressure measured. Irritation of the ring of Vieussens caused an increase of the pressure amounting to only from thirty to seventy mm. Hg.; while in the carotid it was only five to ten mm. In the vena porta the increase of pressure was ten to twelve mm. Division of the ring of Vieussens caused a diminution of the pressure in the hepatic artery, which reached its maximum only after ten to fifteen minutes.

From these facts the authors consider that the artificial production of diabetes is due to dilatation of the liver-arteries and an increased flow of blood to the liver. The failure to produce diabetes by division of the splanchnics or of the main trunk must be explained by the fact that the latter operation likewise dilates the blood-vessels of all the abdominal organs, and thus dilatation of the arteries of the liver does not produce excessive flow of blood to that organ, because a large portion of it goes to other distant organs.

One difference between diabetes caused by pricking the fourth ventricle and that observed by the authors is found in the presence of hydruria in the former case. This is probably explained by the pricking causing likewise dilatation of the vessels of the kidneys.

Jolly on the Dangers of Chloral Hydrate. [Bayer. Aertz. Int. Bl., 1872, No. 12.]—In the course of two years, during which the author employed chloral hydrate in diseases of the mind, he observed two cases in which death occurred suddenly after the use of five grammes at one dose, which is much below the ordinarily-employed maximum doses; the preparation presented in both cases all the characteristics of chemical purity. The psychical phenomena in both patients were preceded by a state of acute excitation, without any symptoms of paralysis, in one case from abuse of alcohol, in the other from unknown etiological causes; during life the patients presented no contraindication against the employment of chloral. The one, after receiving for four days at evening 5.0 grammes, expired with the fifth, a few moments after the administration of a similar dose, with instantaneous stand-still of the movements of the heart and of respiration; the *post mortem* showed anæmia of the brain, acute œdema of the lungs, abnormal congestion of lower abdominal organs, completely normal heart, the vessels intact, blood dark and thin. The other had for twelve days received 5.0 grammes, with normal hypnotic effects, after a short stage of excitation; on the thirteenth day he sank a quarter of an hour after the administration of a similar dose, and expired after a few rattling inspirations. The *post mortem* in this case presented only an extensive œdema of the lungs, thin fluid blood which was normally distributed in the organs; the heart, however, was large and flaccid, its muscle pale but not friable. In concluding these observations of his own, the author sums up all that is known thus far of the toxical phenomena caused by the continued use of chloral hydrate, and attributes them to the following three causes which have been proved experimentally to follow the use of chloral: 1. To the retardation of respiration, which may finally become stertorous, and cause stupor, coma, and death. 2. To the paralyzing influence upon the vaso-motor nervous system (the insufficient innervation of the cutaneous vessels produces erythema, petechiæ, etc., in the use of chloral). 3. To the direct paralyzing action upon the heart itself and its contractibility. The sudden occurrence of this last cause of death by paralysis of the heart was also present in the author's cases.

New York Dispensaries.—The number of free patients treated in the subjoined dispensaries in New York City, during the year 1871, is as follows:

New York Dispensary, corner of White and Centre Streets......	35,348
Northern Dispensary, Christopher Street and Waverley Place...	18,217
Northwestern Dispensary, corner of Thirty-sixth Street and Ninth Avenue..	13,421
German Dispensary, 8 Third Street...........................	16,121
Demilt Dispensary, Second Avenue and East Twenty-third Street	26,654
Eastern Dispensary, 57 Essex Street, corner of Grand...........	25,880
Northeastern Dispensary, 222 East Fifty-ninth Street, near Third Avenue..	19,703
Bureau of Surgical and Medical Relief at Bellevue Hospital for Out-door Poor..	17,700
Harlem Dispensary, Fourth Avenue, between One Hundred and Twenty-sixth and One Hundred and Twenty-seventh Streets..	3,632
Central Dispensary, 934 Eighth Avenue......................	5,708
Dispensary and Infirmary, Church of the Holy Trinity, Madison Avenue, corner of Forty-second Street.....................	2,402
Union Dispensary, 246 East Thirteenth Street.................	6,044
New York Eye and Ear Infirmary, Second Avenue, corner Thirteenth Street..	9,287
Ophthalmic and Aural Institute, 46 East Twelfth Street.	3,112
Manhattan Eye and Ear Hospital, 233 East Thirty-fourth Street.	1,958
New York Infirmary for Women and Children, 128 Second Avenue..	5,001
Western Dispensary for Women and Children, 249 Ninth Avenue	1,596
New York Free Dispensary for Sick Children, 406 East Fifteenth Street..	1,962
New York Orthopedic Dispensary, 945 Sixth Avenue...........	935
New York Tumor Dispensary, 101 East Thirtieth Street, corner of Fourth Avenue..	42
Dental Infirmary, corner of Twenty-first Street and Broadway...	5,128
Total number treated................................	219,851

Estimating the population at one million inhabitants (a high estimate), more than one-fifth are *pauper* patients, or *would-be* ones—or 21.98 per cent. Besides these, thousands are gratuitously treated in the various hospitals and at college cliniques.—*Medical Record.*

Appointments, Honors, etc.—The University of Munich, at its recent four hundredth anniversary, conferred upon Mr. Simon the honorary degree of Doctor of Medicine. Dr. Evory

Kennedy, of Dublin, the eminent obstetrician, it is stated, is to run for Parliament at the coming election in Derry. Prof. Nélaton is said to be beguiling his time in translating the "Odyssey." He is afflicted with an incurable disease, an internal tumor. Among the candidates who lately passed the anatomical examination in the University of Berlin with special approbation, was a Japanese student named Sasumi Satoo, son of the private physician of the Mikado. Dr. John Ordronaux is appointed one of the Faculty in the Law School of the Boston University. Dr. J. R. Baudrey, Physician to the St. Vincent Asylum of St. Louis, has received an appointment to the chair of Psychological Medicine in the Missouri Medical College. Dr. John H. Callendar, Superintendent of the Tennessee Asylum, is to lecture upon "Insanity; its Causes and Treatment," in the Medical Department of the University of Nashville. Dr. C. H. Hughes has resigned the position of Superintendent of the Missouri State Asylum. He will give his special attention to the treatment of diseases of the nervous system, in St. Louis. Dr. T. A. Howard is appointed to the superintendency made vacant by the resignation of Dr. Hughes. Dr. Anson P. Hooker, of East Cambridge, has been appointed Assistant Surgeon-General of Massachusetts, by his Excellency Governor Washburn.

Steffen on the Æthylid-chloride. [Berl. Klin. Woch., No. 6, 72.]—Steffen has employed the æthylid-chloride in twenty cases in children, and in one adult case. The inhalation occurred as with chloroform. Children resist it, on account of its pleasanter and milder smell, far less, and are rendered insensible in two or at the most three minutes. Where the patients were not restless before the inhalation, there was neither increase of the heart's-action nor alienation of the respiration. Its action is shorter than that of chloroform, hence in protracted operations the inhalation must be repeated more frequently. For children in short operations, three to four grammes suffice. On the other hand, in the extirpation of a mammary carcinoma in an adult, about thirty grammes were needed. The awakening from the narcosis occurs quickly, and without any disagreeable sensation. Vomiting very rarely occurs with children. The author decidedly prefers the æthylid-chloride to chloroform, because of its rapid and prompt action, its greater safety, the rapid recovery from the insensibility, and the absence of unpleasant after-effects. The only objection against it lies in its high price.

Wurm on the Diphtheria Question. [Blatt. f. Heilwiss., 7, 72.]—Dr. Wurm makes the following remarks: "It is unfortunately incomprehensible to me, after an experience wanting neither in quality nor quantity, how physicians of celebrity can repudiate the sovereign means of timely, energetic, and persistent cauterizations in diphtheria, instead of giving this means the greatest possible popularity." As Dr. Wurm's thesis contains nothing otherwise essential, we will merely give his opinions on this point: He has until now, without exception, always effected a rapid convalescence by means of timely cauterizations with muriatic acid, which were repeated every one to three hours.

In the "murderous epidemic" all non-cauterized children died, while three which were well "touched" are well to-day. There follows the application, apparently as a result of it, a striking remission and euphoria, with moderation of the pulse, etc. Dr. Wurm complains that he has not time to give more than a glimpse of the clinical course of each case, as he has three hundred bath-guests to attend from 5 A. M. to 10 P. M. He has only found time to affirm the above proposition with great earnestness, because three children who were "touched" are alive and well to-day.

Bellevue Hospital, N. Y.—Provision is made for 1,200 beds. The admission of patients, from 10 A. M. to 3 P. M., is readily procurable upon the recommendation of a physician. From five to six hundred births occur annually in the lying-in wards, of which one-third are illegitimate. Number of patients in hospital January 1, 1872, 779; admissions during the year, 6,359; births during the year, 376; total number treated during the year, 7,514; discharged or relieved, 5,753; deaths, 1,102; remaining December 31, 1871, 659.

House Staff, April 1, 1872.—*House Physicians*—Drs. A.

A. Smith, W. H. Katzenback, F. P. Kinnicutt. *Senior Assistants*—Drs. M. B. Early, J. F. Corrigan, H. S. Swan. *Junior Assistants*—Drs. D. M. McMasters, L. Bolton Bangs, J. C. Lindsly. *House Surgeons*—Drs. A. L. Ranney, J. W. Mitchell, T. A. McBride. *Senior Assistants*—Drs. W. B. Dunning, W. F. Fluhrer, G. A. Van Wagenen. *Junior Assistants*—Drs. W. T. Bull, J. D. Griffith, T. H. Burchard. *Provisional Junior Assistants*—Drs. M. H. Forrest, J. L. Perry, Leroy T. Brooks, W. H. Farrington, M. Figueira. *Examining Physician*—Dr. P. B. Wyckoff. *Assistant Examining Physician*—Dr. C. Ferriberry. *Ambulance Surgeons*—Drs. C. H. Greenough, J. M. Delgade.

Dispensaries in Brooklyn, N. Y.—The number of poor patients treated, in 1871, in the following dispensaries, etc., was as follows:

Brooklyn Central Dispensary, 812 Raymond Street	4,672
Brooklyn Dispensary and Ear Infirmary, 11 Tillary Street	8,107
Dispensary of the Church Charity, Foundation Out-door Department of St. John's Hospital, 1702 Fulton Avenue	1,450
Brooklyn Orthopedic Infirmary, Raymond Street near De Kalb Avenue (Brooklyn City Hospital)	857
Williamsburgh Dispensary, 161 Fourth Street, E. D.	7,642
Brooklyn Eye and Ear Hospital, 208 Washington Street	1,937
Total number treated	24,665

Being 6.16 per cent. of the whole population.—*Medical Record.*

Ploss on the Posture of the Woman during Labor. [Centralbl. d. Med. Wiss., No. 27.]—With the assistance of a mass of very carefully gathered material, the author reviews the various modes of lying, sitting, squatting, hanging, and standing, as adopted by different races for the women in the act of parturition. In some places the reports are so complete as to present very fairly a chart of the different positions, and the passage of a favorite position from one people to another is often traced with certainty. Generally in the choice of a position with the people of antiquity, the attempt appears to have been to hasten the expulsion of the fœtus by all possible complete employment of the factors of expulsion, especially abdominal pressure. Whether original differences in the structure of the pelvis entered into the calculation cannot be determined, because of the scarcity of material. The proof is also absent as to whether one or another position is capable of accelerating labor.

Progressive Muscular Hypertrophy. BENEDIKT. (Wiener Med. Presse, 1872, 9.)—The interest in the case lies in the patient being an adult, that the shoulder-muscles were those affected; there was extensive vaso-motor paralysis in the face, neck, and breast, and over the shoulder-blade, which quickly disappeared on galvanization of the cervical sympathetic. The reaction on opening the current with a gradually-increasing current at the anode became weaker or disappeared, to reappear again when the current became stronger.

The Philadelphia Medical Times.—This excellent and enterprising journal announces an important change in its issue, beginning with the number for October 1st, with which it enters the third year of publication. From that date the *Times* will be a weekly instead of a bi-weekly journal, and the price of subscription will be raised from four to five dollars per annum. We hope the experiment may be abundantly successful.

Atrophia Neurotica. BENEDIKT. (Wiener Med. Presse, 1872, 9.)—This disease, which is more frequently seen to affect the face, is characterized by atrophy of the muscles, bone, and generally of the skin, without paralysis. In the present case, a girl, sixteen years old, the bones of one foot are symmetrically smaller than the other, and those of the leg are shorter. The atrophy of the muscles reaches the thigh; the skin is not yet changed.

American Medical Association.—The triennial list of permanent members will be published this year. Permanent members who have not paid their assessment will please notice:

"Any permanent member who shall fail to pay his annual dues for *three successive years*, unless absent from the country, shall be dropped from the roll of permanent members."

WM. B. ATKINSON, *Permanent Secretary.*

British Medical Association.—The next meeting of this Association will be held at Bradford, September, 1873. Belfast is the place chosen for 1874. Dr. James Prescott Joule is the President-elect. The tenth annual meeting of the British Pharmaceutical Conference will also be held in Bradford next year.

Lady-Doctors in Russia.—Three hundred young Russian women have applied to be enrolled as students in the special courses of medicine and surgery lately established for females by the Academy of Medicine. It is stated, however, that the number of admissions is restricted to seventy.—*Lancet.*

Association of Medical Superintendents.—The Twenty-seventh Annual Meeting of the Association of Medical Superintendents of American Institutions for the Insane will be held in Baltimore, on the fourth Monday in May, 1873.

Worcester City Hospital, Mass.—By the will of the late George Jaques, of Worcester, Mass, the sum of a quarter of a million dollars was bequeathed to the city of Worcester for the new Free City Hospital.

The Syracuse College.—The College of Physicians and Surgeons of Syracuse University opened on the 3d instant, with a full corps of eighteen professors.

Mercury among the Ancients.—Some give Paracelsus the credit of being the first to use mercury in syphilis. This seems to be a mistake. According to Boerhaave, Berengarius was the discoverer of its merits; and the following is from Douglas's "Bibliographia Anatomica," Lyons, 1734: "Jacobus Berengarius Carpensis, ita dictus a Carpi civitate in Italia. . . . innunctionis ex hydrargyro in curâ luis vennereæ primis fuit inventor, illoque solo quæstu mirè oppulentus redditus est."

Intestinal Obstruction.—In Culpepper's translation of "Riverius," London, 1678, reference is made in the two hundred and twenty-sixth observation to a case of obstruction where the contents of the gut made their way out into the abdomen above the stricture, and again by an ulcerated opening into the rectum. "Thus a looseness happening in the iliac passion may deceive a physician."

Obituary.

The late Prof. Edward Parrish, of the Philadelphia College of Pharmacy, who died September 9th, at Fort Sill, Indian Territory, of typho-malarial fever, was a son of the celebrated Dr. Joseph Parrish, and was born in 1822. After graduating in medicine, he was appointed a professor in the Philadelphia College of Pharmacy, with which institution he has ever since been connected. He was also for a time President of Swarthmore College. Prof. Parrish was the author of many medical essays and works, the principal one of which was "Parrish's Pharmacy." At the time of his death he was visiting the Western Territories as a special peace commissioner to the Kiowa Indians.—*Medical Times.*

Dr. John Bell.—This old and eminent Philadelphia physician died August 24, 1872. He was at one time an editor of a Philadelphia journal, President of the American Colonization Society, and for one session Professor of the Theory and Practice of Medicine in the Ohio Medical College. He was also one of the authors of "Stokes and Bell's Practice," a standard work in its day, and the writer of a prize essay on "The Influence of Alcohol on the Production of Tuberculosis."

M. Louis.—The death of this eminent and veteran physician and teacher occurred in Paris, on the 23d of August, after an illness of about two months. His name is associated with some of the most important pathological discoveries of this century. He graduated in medicine in 1818. He was eighty-six years of age at the time of his death.

Dr. Henry Curran, L. R. C. P. I., of Dublin, died in August, from the effects of typhoid fever. This successful teacher was, for many years previous to his death, connected with the Carmichael School of Medicine.

Prof. Stober, of Strasbourg, well known as an expert oculist, is deceased.

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ART. I.—*On Ovariotomy.* By J. MARION SIMS, M. D., one of the Surgeons to the New York State Woman's Hospital, etc., etc.

WHEN Clay, of Manchester, and Peaslee and the brothers Atlee, of America, began to perform ovariotomy, they met with nothing but rebuke from their brethren, who stood aghast, and called them by the hardest of names. I was among the great herd that could scarcely find terms strong enough to condemn what were then characterized as acts of butchery and murder. But, thanks to their unfaltering perseverance, seconded eventually so ably by Spencer Wells and Baker Brown in England, and by Kimball, Dunlap, and Bradford in our own country, ovariotomy is to-day recognized by the profession everywhere as a legitimate and justifiable operation.

Twenty years ago I knew personally no one who was in favor of it; and now I can find no one opposed to it; and this complete revolution has been the work of but eight or ten years. It has become our habit of late to boast of the success of the operation. We pretend that we have perfected the method of operating, and that we are gradually curing a greater percentage from year to year. For myself I do not believe our method is perfect; and, while I must acknowledge that in the hands of a few skilled operators the mortality has been slightly reduced, I am frank to say that even with their results I am not at all satisfied. I do not believe that we are very far in advance of the earlier operators; for if the whole truth were known, if every case operated upon saw the light of day, I doubt if we have made any great progress since the time of McDowell, the father of Ovariotomy.

Dr. Thomas[1] gives us a table of operations performed by twenty-five surgeons, beginning with Spencer Wells's 400 cases. These twenty-five have performed 1,638 operations, and lost 504 cases, giving a mortality of about 1 in 3¼.

I must repeat that with this I am not satisfied, and that we have made little or no progress in the last ten years. One great principle has been pretty generally adopted, that of securing the pedicle externally. Duffin is, I believe, the author of it; Erichsen and Hutchinson were among the first to adopt it; but Spencer Wells and Baker Brown, and afterward Keith and Kœberlé and Atlee and Kimball, have made it almost the universal practice.

It renders the operation easier and quicker, but its advocates have no better success than Clay and Tyler Smith, and Peaslee and others, who follow a different method. The clamp may have its advantages; but it also has its disadvantages, which more than counterbalance the former. It is not of universal applicability, for in some cases the pedicle is too short and thick to allow of its use at all. In others, it is obliged to be removed prematurely, on account of traction and consequent suffering. In some it has slipped a little and allowed bleeding. In a few it has severed the pedicle too soon and allowed it to drop into the peritoneal cavity before adhesions were formed to fix it to the abdominal walls. In other instances, its traction and pressure have compelled its removal, and the short, stumpy pedicle has dropped in, with sloughy shreds attached, to poison the peritoneal membrane. Its traction produces often great suffering, necessitating the use of large and repeated doses of opium; and it doubtless aggravates, by reflex action, the tendency to nausea and vomiting; and who can tell what influence it may have exerted in such cases as have died of tetanus? Besides all this, the pedicle attached to the abdominal wall, and other bands of connection between this and the omentum, have been the means of intestinal strangulation and of death.

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[1] "Diseases of Women," third edition, p. 719.

A good result, that has not unfrequently followed its use, is that, in the act of vomiting, poisonous fluids have been accidentally forced out of the peritoneal cavity beside the pedicle, and thus lives have been unwittingly saved that otherwise would have swollen the already too great mortality.

The management of the pedicle is still a mooted point, which must ere long be settled. I think the clamp plan has seen its best days. When the question is positively determined, I have scarcely a doubt that it will be against the Duffin principle of drawing the pedicle externally. Clay adopts the principle, laid down by the great McDowell, of ligating the pedicle with a strong twine and leaving it dependent from the lower angle of the wound. Thus it forms a canal or outlet for drainage which has often saved life. Tyler Smith and Peaslee have been in the habit of tying the pedicle with a strong ligature, of cutting it off short, and dropping it into the peritoneal cavity, and of wholly closing up the external wound; and their success is quite equal to that of the clampists—for they have lost 1 in $3\frac{7}{18}$, while the others taken collectively have lost 1 in $3\frac{7}{18}$, a difference showing that it makes no difference whether the pedicle is managed in one way or another. Of course, I know that such statistics are valueless, and that all statistics on a small scale are utterly so. For instance, I have known Mr. Spencer Wells to lose seven cases in succession, which is no argument against the clamp; and again, I have known him to perform more than twenty operations without a fatal case, which, offset by the others, is no argument in favor of it.

For twenty years I have advocated the plan of tying the pedicle with silver wire, which I felt sure would become sacculated, and therefore produce no harm; and for the last ten years this plan has been followed by Dr. Emmet and myself. When the pedicle was narrow, I transfixed it with a double wire, and tightly twisted a wire around each half of the pedicle. When the pedicle was broad, I introduced the requisite number of separate wires and secured it in segments. In one case, in Paris, I used six separate wires. But sometimes the wires, when tightened, have torn the segments asunder and produced a little bleeding, thus necessitating an additional wire, at considerable loss of time. Dr. Emmet, seeing this difficulty, then hit upon the happy idea of securing the pedicle by a figure-of-8 loop of wire and drawing the whole so firmly that, when the wires were fastened (by twisting) and the pedicle cut off, the ends of the constricted arteries could often be seen projecting beyond the level of the cut stump. The wire, thus applied, effectually strangulates the blood-vessels and prevents all danger of bleeding. It burrows into the tissue, and soon becomes perfectly sacculated, where it always remains without danger or discomfort. In 1864 I assisted Mr. Nélaton and Sir Joseph Olliffe in an ovariotomy in Paris. I told Mr. Nélaton how I managed the pedicle, and he kindly asked me to apply the wire ligature for him. The pedicle was long and slender, and I passed a double wire ligature and twisted each one tightly around its respective half of the pedicle. This was cut off nearly an inch beyond the point of constriction, and I then drew the serous membrane over the stump of the pedicle, just as we would the skin over the end of an arm or leg amputated by the circular method, and I whipped this membrane over with a continuous very fine wire suture. By this method no cut surface was left in contact with the peritoneal membrane.[1] It was simply serous tissue against serous tissue. Of course, this prolonged the operation at least ten or twelve minutes. The patient rallied, but died of septicæmia in about forty hours. On *post-mortem* examination we found, what I expected, the pelvis filled with about a quart or more of reddish serous fluid, which was the septicæmic source. But fortunately the patient lived long enough for Nature to make her own disposition of the wires in the pedicle. We found the pedicle floating loosely in the sero-sanguinolent fluid, and no sign of either of the wires could be seen. The strong, constricting wire had cut through the serous membrane, and this had healed over it, and it was thus completely sacculated, hidden from view. The little fine wire suture, with which the end of the stump had been closed up, was also lost to view, and it was necessary to incise the tissue to find either this or the constricting wire. Nothing could possibly have been more satisfactory.

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[1] A procedure now proved to be unnecessary.

So far, Dr. Emmet and myself have every reason to be satisfied with our method of dealing with the pedicle. But I have no prejudices whatever on the subject, and I think the time may possibly come when clamps and ligatures, whether of twine, cat-gut, or wire, may give way to torsion of the arteries, or to their obliteration by the enucleation of the pedicle from the coats of the cyst.

It must be admitted that torsion of the arteries of the pedicle is an important improvement in the operation of ovariotomy, and for this we are indebted to Dr. G. D. Bebee,[1] of Chicago. Dr. Bebee divides the pedicle not at one full sweep of the scissors, but, severing a portion at a time, the vessels are seized and twisted as soon as divided. He had used torsion successfully in six cases, up to the time of his report. And why should we not twist the arteries of the pedicle? In Guy's Hospital, torsion is the rule in all amputations and exsections. They have not tied an artery there in the last four or five years. At Sedan, I twisted, for MacCormac, arteries in all sorts of operations, and never with any unhappy result.

In his three amputations at the hip-joint, I twisted the arteries as fearlessly and as confidently as I did those of the wrist or the ankle, and with the same impunity. When I was at Guy's Hospital in August, 1870, and saw them torsionizing arteries, even in a hip-joint amputation, I said to one of the surgeons, "Have you twisted the arteries of the pedicle in ovariotomy?" He replied, "No," and in a manner that implied more; and yet for three years he and his colleagues had been daily using torsion, to the entire exclusion of the ligature. How hard is it for us to overcome the surgical dread of the peritoneal cavity handed down to us from the old masters!

Dr. Miner's[2] method of enucleating the tumor from the expansion of the pedicle seems to find favor with the profession, and very justly too. It is as follows: "The finger should be gently introduced under the central portion of the pedicle, and followed out along the fasciculi of vessels as they extend over the sides of the cyst. Nothing could be more easy of execution, or more readily accomplished." I have tried Dr. Miner's method, and have but little doubt that it will come into general use. Indeed, I look upon it as a most valuable improvement. Even the advocates of the clamp must adopt it, as it will make the shortest pedicle quite long enough for their use. We shall then have no more complaints that the patient died because the pedicle was so short that it could not be drawn out and secured by the clamp.

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[1] *American Journal of Medical Science*, April, 1871, p. 353.

[2] *Medical Record*, March 1, 1872, p. 14, and *American Journal of Medical Science*, October, 1872, p. 391.

Dr. John L. Atlee, many years ago, used Chassaignac's *écraseur* successfully for severing the pedicle; and recently Dr. Nott's rectilinear *écraseur* has been used for the same purpose, and with perfect success. Mr. Baker Brown used the actual cautery in 1864 and 1865. But the profession did not adopt it. However, I hear, through Dr. Dawson, just returned from London, that Dr. Meadows has been lately very successful with it at the Soho Square Hospital for Women. The transition from the hot iron to the platinum wire heated by electricity is natural enough, and the electro-cautery has been called into requisition by Dr. Noeggerath and others. But something more must yet be done before we can claim to have made any very decided improvement in the operation of ovariotomy, or to have diminished to any notable extent the degree of mortality now attending it.

It seems that the whole power of the medical mind, ever since the days of McDowell, has been concentrated upon the pedicle, and that its resources have at length been exhausted. As the pedicle was the source of nourishment to the diseased mass, it was looked upon as the great enemy to be conquered. And when it was eventually drawn out of its natural stronghold, and placed in chains and manacles, an "*Io! triumphe!*" was suddenly shouted out by the whole profession, and men everywhere rushed forth madly to ovariotomize women, sometimes those who unfortunately had only fibroids or spleens, or kidneys, or even phantom tumors, to be extirpated! It was not so much the success of Spencer Wells and Baker Brown, that fired the profession, as it was the thought that the supposed great enemy to success (the pedicle) had been led captive. The fact is, we have been so elated with what we have deemed a great achievement, that we have forgotten or overlooked all other sources of danger. We have in our imaginations created a hideous bugbear of the poor pedicle, and we have rushed upon it in a most frantically Quixotic manner.

Indeed, it really seems as if we had gone mad on the management of the pedicle; and still the question is an open one.

In all candor I think we have done enough and more than enough for the pedicle, and it is high time to leave it for a while, and to turn our attention to some other quarter, if we are ever to reduce the mortality of this operation from what it has been and even is to-day. And now to the question:

I have not seen many ovariotomy *post-mortem* examinations, but the few that I have seen presented uniformly the same general pathological appearances.

I have recently been studying Mr. Spencer Wells's[1] first volume, giving an account of his first hundred and fifteen operations. This volume is in itself a mine of wealth on this important subject. I have patiently examined the records of his thirty-nine fatal cases, with their *post-mortem* appearances, and I find that my limited observation is fully sustained by his immense experience. Our earlier operators seemed to think that hæmorrhage and peritonitis were the chief sources of danger. But nowadays how rarely does a patient die of hæmorrhage, and very few die of peritonitis, properly speaking. If none died but of peritonitis, I am satisfied that the mortality would not amount to one in ten. I fear that the causes of death are too often based upon preconceived opinions or guessed at without *post mortems*. In looking over some reports of cases in the medical journals taken at random, I find such items as these: "She progressed favorably till the third day, when symptoms of secondary hæmorrhage and peritonitis appeared, and death occurred on the day following. No *post-mortem* examination was made."

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[1] "Diseases of the Ovaries." By T. Spencer Wells, F. R. C. S., etc., etc. London, 1865.

Again: "On the second day after the operation, symptoms of peritonitis manifested themselves, and the patient died on the fifth day. No *post mortem.*" And again: "She gradually sank, and died in thirty-six hours, with symptoms of peritonitis. Impossible to obtain a *post-mortem* examination."

Now, I have given these veritable extracts to show that our statistics are as yet of no great value in giving us the real causes of death after ovariotomy.

Dr. John Clay,[1] in his analysis of the causes of death in 150 cases gives the following:

Shock or collapse	25	=150.
Hæmorrhage	24	
Peritonitis	64	
Not stated	18	
Other causes	19	

Dr. Peaslee[2] gives the following table of 51 cases, collected by himself:

Peritonitis	12	Diarrhœa	1
Septicæmia	9	Erysipelas	1
Shock and collapse	7	Tetanus	1
Exhaustion	7	Ulcer through the bladder	1
Shock and septicæmia	1	Unknown	9
Hæmorrhage	1		
Strangulation of intestine in wound	1		

In Mr. Spencer Wells's[3] table of 500 cases of completed ovariotomy, we find recorded 128 deaths, classified as follows:

Peritonitis	44	Cancer	1
Exhaustion	22	Peritonitis and heart-clot.	1
Septicæmia	21	Chronic peritonitis from an accident	1
Collapse	7	Shock	1
Intestinal obstruction	4	Pneumonic congestion and embolism	1
Heart-clot	3	Coma from heart-disease.	1
Septic peritonitis	3	Hyperpyrexia	1
Tetanus	2	Pneumonic congestion	1
Diffuse peritonitis	2	Pleuritic effusion	1
Pyæmic fever	2	Hyperpyrexia and pericarditis	1
Pulmonary embolism	2	Pneumonia	1
Cardiac embolism	2		
Pyæmic pleurisy	1		
Crural phlebitis and septicæmia	1		
Chronic peritonitis	1	Total	128

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[1] Thomas, "Diseases of Women," third edition, p. 716.

[2] "Ovarian Tumors," p. 348.

[3] "Diseases of the Ovaries, their Diagnosis and Treatment." By T. Spencer Wells, etc. London: J. & A. Churchill. New York: D. Appleton & Co., 1872.

Dr. Peaslee's and Mr. Wells's tables are of more recent date than Dr. Clay's, and we find them giving septicæmia as a cause of death, while it is not mentioned by Dr. Clay.

Septicæmia is, I think, the great outlet of life in ovariotomy. What the sources of this septicæmia, and what the treatment, prophylactic and curative, is the object of this paper.

I have seen seven *post-mortem* examinations, and in each one there was found in the peritoneal cavity a grayish, turbid serum or a sero-sanguinolent fluid, varying in quantity from four ounces to as many pints, and yet there was no general peritonitis.

Of course, this sero-sanguinolent fluid is more apt to be found where there are extensive adhesions; but it is often the mode or cause of death where there are no adhesions at all. It is known and generally acknowledged that adhesions between the tumor and the abdominal parietes, or between it and the viscera, always add to the risk of the operation; and yet we see patients sometimes recovering from serious operations where there were extensive adhesions, and that with very little constitutional disturbance.

If the adhesions are broken up, leaving a dry surface, the danger is less; but if the adhering surfaces are disposed to ooze a little blood—if there should be a sort of bloody sweating, as it were—then the danger is greater.

Now, to prove that my views of pathology here are correct, I propose to take up Mr. Spencer Wells's thirty-nine fatal cases, and to show that in the main and in detail they fully sustain me.

I have selected Mr. Wells, because there is no higher recognized authority among us, and because his cases are reported minutely and without any reference to any theoretical views whatever. Indeed, he calls many of his cases peritonitis which I shall try to prove by himself were not peritonitis at all.

I shall take his cases *seriatim*, from one to thirty-nine, designating each by its proper number in Roman letters as it stands in his book. I shall affix the date of operation in each case, the time of death after operation, the *post-obit* appearances as far as they bear upon the question at issue; and, when any little item in the history or progress of the case may tend to throw light on the subject, I shall appropriate it—and always in the identical language of Mr. Wells's model book. I shall give his opinion upon the cause or causes of death, whenever he has expressed it, and I shall take the liberty of submitting my own opinion, based upon the symptoms and pathological appearances in each case. I shall take the further liberty of italicizing Mr. Wells's language wherever it suits the purpose of elucidating my own views. I have done this that the reader may, without loss of time, run his eye, in a few minutes, over the important points in the whole thirty-nine cases.

No. 1 (Case IV.).—Operation, January, 1859. Death in thirty-two hours. *Post mortem* by Dr. Aitkin. "The lateral and posterior parts of the abdominal cavity were free from lymphy exudation, and the peritonæum appeared natural. *A considerable amount of free liquid was present in the cavity generally* (as over the anterior margin of the liver, and surface of the stomach and transverse colon); it was pent up within cavities formed by recent exudation. *The fluid exudation was of an acrimonious nature*, if one may judge of its effects upon the hands. *The fluid had a pungent, irritant effect upon the thin skin beneath the edges of the nails and surrounding thin matrices.*

No. 2 (Case VI.).—Operation, January, 1859. Death in forty hours. First day after operation. "*The bed was saturated with ascitic fluid, which continued to dribble beside the clamp.*" *Post mortem* three hours after death. "*There were from two to three pints of clear serum in the peritoneal cavity*, no blood nor clots. There was evidence of peritonitis to a considerable extent in the parietal portion of the membrane. The peritonitis did not appear to have extended to the more deeply-situated folds of the intestine. *About a pint of serous fluid had gravitated into the pelvic cavity.* In the right pleural cavity there were upward of six pints of clear serum."

No. 3 (Case X.).—Operation, October, 1859. Death fourth day. On the third day "*a good deal of sanious discharge came away from the lower part of the wound.*" *Post mortem* by Dr. Aitkin and Mr. Huxtable. The folds of intestine near the wound were united together by lymph effused at the spots where the folds came into contact with each other. There was no recent lymph, and *only about a pint of serum in the most dependent parts of the peritoneal cavity.*

No. 4 (Case XII.).—This case died of tetanus on the tenth day after the operation.

No. 5 (Case XIV.).—Operation, December, 1859. Died in twenty-three hours. "The abdomen was examined on the following day. *There were between one and two pints of bloody serum in the peritoneal cavity*, but not a morsel of clot. There were signs of peritonitis about the broad ligament on the left side, and on the parietes near the wound, and on two or three coils of intestine that lay near it; *but there were no marks of general peritonitis.* The ligatures did not appear to have set up peritonitis in their track."

No. 6 (Case XVI.).—Operation, February, 1860. Death in thirty hours. No *post-mortem* examination was allowed. But Mr. Wells says: "I am disposed to attribute the death in this case partly to imperfect recovery from the shock of the operation, and the consequent exhaustion, and partly to the absorption of some morbid product of the decomposing cyst."

No. 7 (Case XVII.).—Operation, February, 1860. Death in forty-six hours. *Post-mortem* examination, twenty hours after death, by Dr. Aitkin. Constriction of the lower portion of the jejunum (by pedicle), which was dilated and inflamed on either side of the constricted portion.

Extensive inflammatory action had glued the convolutions of intestine to each other immediately above the constricted portion, and "*a considerable portion of fluid effusion filled the cavity of the true pelvis.*"

No. 8 (Case XXIV.).—Operation, April, 1861. Death in twenty-four hours. Examination, twenty-four hours after death. "*The peritoneal cavity contained from four to five pints of reddish serum*, but no blood-clot, no lymph, nor adhesions. The serum found in the peritoneal cavity must have been a very active animal poison, for I" (Mr. Spencer Wells) "suffered severely two days after the examination from a very slight scratch with the point of a needle on the left forefinger. I sucked the spot instantly, but the next day a small vesicle formed and I applied caustic freely. On the second day I had some rigors, lasting several hours, with intense headache; relieved by vomiting and a copious perspiration, which lasted about eighteen hours. For several days afterward I was very weak, but all the severe symptoms had passed off by the fifth day after the puncture." Mr. Wells further says: "To the report of this case published in the *Medical Times and Gazette*, in May, 1861, I made the following remarks, and subsequent experience has tended to support the views I then brought before the profession: 'This recalls a question I have raised before. *The peritonæum contained some pints of poisonous serum.* It was probably formed by part of the membrane, and might be absorbed by other parts. If so, a poison which affected me so severely in a small dose, might easily kill any one in a larger dose. I recovered after the absorption of a fraction of a drop; but the poor woman was overpowered by the quantity taken up by her own absorbents.'" This case seemed particularly to point Mr. Wells's mind in the right direction of inquiry, for he says, farther on: "This is not a mere question of theory or curiosity, for it leads to an important rule in practice, or rather to the suggestion that, in cases *where such poisonous serum may reasonably be supposed to be present*, a part of the wound should be opened to allow the free escape of the serum by the side of the peduncle. In two cases formerly published, I acted upon this rule with the greatest advantage, and both patients recovered. . . . I am disposed to think that, in many cases where there is such a condition as I have described after operation, a free opening should be made for the escape of the serum."

No. 9 (Case XXV.).—Operation, June, 1861. Death on the fifth day. "The patient went on well for four days, then began to sink very suddenly and died. At the *post-mortem* examination some recent lymph on the anterior surface of the liver was the only sign of peritonitis; *but there was a good deal of turbid serum in the peritoneal cavity*, and the intestines were much inflated. There was no blood in the abdomen. It appeared that death had taken place from simple exhaustion."

No. 10 (Case XXVI.).—Operation, July, 1861. Death in forty-eight hours. "The *post mortem* showed nothing but *turbid serum in the peritoneal cavity*, and intestines distended with gas; no blood nor clot. It was clear that death took place from exhaustion, partly the result of the uncontrollable vomiting."

No. 11 (Case XXIX.).—Operation, October, 1861. Death in forty-seven hours. "*There was some oozing of bloody serum*, and a little blood from the stump; so, in the afternoon of the day after the operation, I reapplied the clamp close to the ligature. She died, collapsed, in forty-seven hours. At the *post-mortem* examination we observed proofs of extensive peritonitis, both recent and of old date. There was not a drop of blood, nor any clot, in the abdominal cavity. The marks of peritonitis were not most intense around the operated parts, but in the neighborhood of the liver."

No. 12 (Case XXXI.).—Operation, December, 1861. Death on the twelfth day. A good deal of oozing of blood before closing of wound, but no vessels ligated. Sutures and clamp removed on the fourth day. Constant nausea. On the seventh day, Dr. Lawford says: "The abdomen is enormously distended, and last evening, during an attack of sickness, the *upper portion of the wound burst open.* I replaced the sutures a few minutes afterward."

Eleventh Day.—Dr. Lawford wrote to Mr. Mills: "The ligature connected with the portion of omentum has not yet come away. I was pulling it this morning, but it would not separate, *but an ounce or more of pure pus escaped from the aperture.*" On the twelfth day the patient died. "No *post-mortem* examination was permitted."

No. 13 (Case XXXII.).—Operation, January, 1862. Death in twenty-nine hours. "On *post-mortem* examination, proofs of general diffuse peritonitis were observed, many coils of intestines being glued together by recent lymph, and the surface of the peritonæum being covered generally by a pasty layer of the albuminous portions of the ovarian fluid. *Some of this fluid had gravitated into the pelvis*, but there was no blood nor blood-clots in the cavity." Mr. Wells, farther on, in speaking of the error of leaving the least ovarian fluid in the peritoneal cavity, as was done in this case, says: "*It is prob-*

ably mixed with blood from separated adhesions, and is pretty sure to putrefy and to poison the patient if she live long enough."

No. 14 (Case XXXIII.). — Operation, January, 1862. Death on the fourth day. *Post-mortem* examination. "*There was a little clear serum in the peritoneal cavity*, but no blood, nor ovarian fluid, nor any trace of peritonitis. It appeared, therefore, that simple exhaustion was the sole cause of death."

No. 15 (Case XXXIV.). — Operation, January, 1862. Death in sixty hours, of septicæmia. No *post mortem*, but Mr. Spencer Wells puts it down as death from septicæmia.

No. 16 (Case XXXV.).—Operation, May, 1862. Death on the fourteenth day, of tetanus. *Post mortem.* "There was no sign of peritonitis."

No. 17 (Case XLV.).—Operation, October, 1862. Death in forty hours. She rallied well, but vomited several times, and *a good deal of reddish serum oozed out from around the pedicle.* Five hours after the operation the clamp was removed, allowing the stump and ligature to sink within the abdomen. "*A good deal of reddish serum then began to ooze away and continued to do so.* She became easier. The pulse was good—90 to 100—and there was no more vomiting." During the day after the operation *a great deal of serum oozed from the abdomen;* she became restless and depressed; the pulse rose to 110, 120, and 135; she continued to sink during the night, and died forty hours after the operation.

Post Mortem.—"*A quantity of serum, tinged with blood, escaped as soon as the peritoneal cavity was opened.* . . . There was neither blood nor clot in the abdomen. . . . The cause of death, therefore," says Mr. Wells, "was extensive diffuse peritonitis of a low-form, and was probably due in a great measure to the unhealthy constitution of the patient."

No. 18 (Case LIV.).—Operation, February, 1863. Death in forty-four hours. On the day after the operation the pulse rose to 140, was more feeble, and she became weaker. "*Early next morning she became faint, and a profuse discharge of serum, upward of a pint, escaped beside the pedicle;*" she continued to sink, and died forty-four hours after operation.

At the *post-mortem* examination "there was not a drop of blood nor any clot in the peritoneal cavity, but there were evidences of a low form of diffuse *peritonitis, shown rather by the effusion of serum, than of lymph.* . . . There was a little *bloody serum in the sub-peritoneal tissue of the uterus and left ovary.*"

No. 19 (Case LIX.).—Operation, March, 1863. Death twenty-six days after operation. On second day after operation, "much flatulent distention of abdomen, pulse 130, clamp drawn backward, depressing the lower part of the wound, so that thirty hours after operation it was removed. The stump and ligature at once sunk inward, and *there was a slight discharge of bloody serum.*" She continued to grow worse, and on the fifth day "there was *a very free, dark, serous discharge beside the pedicle,* and a good deal of tympanitis; but the pulse had fallen to 104." On the morning of the sixth day she was better, had a good appetite, tongue cleaner, pulse 116, "*but the discharge was very free.*" In the afternoon, "*there was a free discharge of bloody serum through the dressing over the wound.*" On removing the dressing to-day, it was discovered that "the skin was not united, the edges gaped widely, and the fat and subjacent tissues bulged up between them; but the peritonæum seemed completely closed;" wound was reunited by sutures. On the ninth, tenth, and eleventh days, she was better; "*the discharge was more purulent, but still fetid.*" On the twelfth day she was very ill. On examination, a soft swelling was found behind the uterus. This was punctured through the vagina with a trocar, and "*a pint of serum, with blood and some pus,* not at all fetid," was removed.

Thirteenth Day.—Free suppuration from wound.

Fifteenth Day.—Wound still disunited down to the peritoneal membrane; in the afternoon a copious discharge of fetid pus escaped by the vagina, and continued.

Sixteenth Day.—Purulent discharge very free by vagina, less so from wound. It is useless to give further daily reports of the case. "She gradually sank, and died on the afternoon of the twenty-sixth day."

Post mortem two days after death, by Mr. Wells and Mr. Cooper. "About three ounces of red serum in the pericardium. No sign of general peritonitis. At the bottom of Douglas's space there was a cavity containing three or four ounces of pus. The opening which I had made into this cavity from the vagina was quite closed. I regretted very much that I had not made it freer and kept it open. A free opening into the vagina should be made and maintained as soon as the existence of fluid is detected."

No. 20 (Case LX.).—Operation, March, 1863. Death in fifty-four hours. She was "apparently exhausted by the vomiting and *the rapid formation of serum in the peritoneal cavity*. On *post-mortem* examination *about forty ounces of dark-red serum*, and two ounces of blood-clot, were found in the cavity."

No. 21 (Case LXV.).—Operation, June, 1863. Death in fifty-four hours. On the second day "there was some prolapse of the pedicle, and *oozing of reddish serum around it*, so that I removed the clamp at 10 A. M. The pulse ran up to 150, and she died fifty-four hours after operation." *Post-mortem* examination twenty-three hours after death. "*Two or three pints of red serum in the peritoneal cavity*, but no clot. There was no pelvic peritonitis. A hard, cylindrical fibrinous clot was attached to the wall of the right ventricle, and passed along the pulmonary artery beyond its first division."

Mr. Wells is rather disposed to attribute the death to the heart-clot.

No. 22 (Case LXVII.).—Operation, June, 1863. Death in eighty hours. No *post-mortem* examination. But Mr. Wells allows Dr. Courty, of Montpellier, to report the case in all its details, who says, "Her death can only be attributed to excessive weakness." But, from the number and extent of adhesions that were broken up, there is no doubt in my mind that she died of *septicæmia*.

No. 23 (Case LXXI.).—Operation, July, 1863. Death in eighty hours. In thirty hours the clamp was removed. On the second day (or between forty and forty-eight hours), "the ligature (and pedicle) had sunk inward, and *there was a free, dark serous discharge from the abdomen*." On the third day, pulse 150. Mr. Wells "inserted an elastic catheter by the side of the ligatures, *through which about six ounces of clear, reddish serum, not fetid, were sucked up by a syringe*."

Patient died eighty hours after the operation. *Post mortem*, by Dr. Barratt, twenty hours after death. "Fibrinous clot in right ventricle. No blood in the peritoneal cavity, *only a little serum and some flaky lymph.*" Mr. Wells puts this case down as "death from fibrinous coagulum in the heart."

No. 24 (Case LXXII.).—Operation, July, 1863. Death in forty-four hours. About thirty-six hours after the operation, Mr. Wells "removed the clamp, after tying a ligature behind it. There *was free oozing of bloody serum*, and a little discharge of pure blood. The pulse rose to 160, and the patient died forty-four hours after the operation."

"The *post-mortem* examination revealed no traces of peritonitis, and no internal hæmorrhage. Death seemed to have been due to want of power."

She evidently died of septicæmia, judging from the *oozing of bloody serum* beside the pedicle.

No. 25 (Case LXXIV.).—Operation, August, 1863. Death eighty-two hours after operation. "A partial examination showed that *the abdomen contained much blackish serum*, and the peritonæum was covered with soft lymph. 'I suppose,' says Dr. Gordon, 'we may say her death was caused by a very low form of peritoneal inflammation.'"

No. 26 (Case LXXV.).—Operation, September, 1863. Death forty hours after operation. "No examination of the body was permitted."

No. 27 (Case LXXVIII.).—Operation, November, 1863. Death on eighth day. On fourth day it was necessary to remove the clamp "after cutting away all the slough above it. The slough sank between the lips of the wound, but seemed to be firmly attached there. At 5 she felt easier, but at 10 the pulse was up to 130; she had vomited again, and was thirsty. *The slough had sunk quite out of sight, and there was some dark serous discharge on the bandage.*

Fifth Day.—"*Some red serum observed on each poultice*, but none appeared at the opening. On passing in the end of the little finger, no slough could be felt, and there was no fetid odor on the finger."

Seventh Day.—"A most careful examination could detect no evidence of pus in the pelvis or cellular tissue."

Eighth Day.—"Patient died. No *post-mortem* examination, but Mr. Wells puts the case down as 'death from septicæmia.'"

No. 28 (Case LXXXI.).—Operation, November, 1863. Death on eighth day. For the first twenty-four hours all went on well, but the pulse varied from 112 to 140. A good deal of abdominal pain and some vomiting. On the second day a dark, sanguineous discharge from the uterus. Third day, diarrhœa. On the fourth and fifth days diarrhœa continued, "and there was a dark, fetid discharge from the umbilicus. Died on the eighth day. *Post-mortem* examination was made by Dr. Ritchie in the presence of Drs. Routh and Rogers.

"The fundus uteri lay on a level between the symphysis pubis and the promontory of the sacrum. A layer of recent gelatinous lymph formed a sort of arched roof from the sacrum and rectum over the uterus to the bladder and pubes, *enclosing about a pint of turbid serum* in the pouch of peritonæum between the uterus and rectum, and to the right side of the uterus. The pouch was lined by a layer of lymph. There was a good deal of serum in the lower cellular tissue of the pelvis. The small slough" (of the stump) "enclosed in the ligatures had been surrounded by two coils of small intestine which were adherent to each other and to the end of the pedicle."

No. 29 (Case LXXXII.).—Operation, December, 1863. Death, sixty-six hours after operation. No *post-mortem* examination was permitted. "On reflecting on this case," says Mr. Wells, "I think it would have been better to keep the ends of the ligature out, and *thus have secured a drainage of fluid from the peritonæum.*"

Mr. Wells does not assign a cause of death in this case, but the above italicized quotation is suggestive, and in my own mind I have no doubt about it.

No. 30 (Case LXXXIII.).—Operation, December, 1863. Death third day. Double ovariotomy. Both pedicles so short that the clamp could not be applied. Each pedicle was tied, and the ligatures were left hanging from the lower end of the wound. On second night after operation "the pulse got quicker, and became intermittent, and stimulants were administered very freely. About 5 A. M., she begged to be turned on her side, when *a good deal of reddish serum escaped* from the wound. At 10 A. M., the pulse was 150, very feeble. At 3 P. M., *there was a little more discharge from the wound*, and she died at 9 P. M. No *post-mortem* examination was permitted."

Mr. Wells assigns no cause of death, but the reddish serum accounts for it.

No. 31 (CASE LXXXIX.).—Operation, April, 1864. Death in one hundred and fourteen hours. On the second day after the operation, "I cut away," says Mr. Wells, "a little slough which was above the clamp, and shortly afterward the pulse fell to 108 (from 120). When I made my evening visit I found that the patient had been sick twice, and that the straining had disturbed the clamp. Some of the stump had slipped through, and this had given rise to a *slight oozing of blood*. I put on a ligature under the clamp, and tightened the clamp again." A bad night, with epigastric tympanites, followed this, and "*there was a very slight serous discharge in the neighborhood of the clamp*," with pulse from 135 to 140, and free perspiration. Patient continued to grow worse, pulse 140 to 160, and on the fifth morning after the operation "I removed the clamp, and the pedicle sank inward, remaining, however, in sight." She "died one hundred and fourteen hours after operation. No *post-mortem* examination of the body was permitted. Dr. Fox's report of the state of the peritoneal coat of the cyst puts the tubercular character of the peritonitis almost beyond question." But, I think, the history of the case after operation puts the fact that she died of septicæmia quite beyond question.

No. 32 (CASE XCII.).—Operation, April, 1864. Death in sixty-four hours. "The patient went on very well for twenty-four hours, but early next morning there was vomiting, tympanites, scanty concentrated urine, and a pulse of 140, and she died at night, exhausted, sixty-four hours after operation."

Post-mortem examination by Dr. Barratt. "*About two pints of dark-red serum had been effused into the peritoneal* *cavity*, which contained neither blood nor ovarian fluid. The recent lymph was confined entirely to the lower and back part of the abdomen and pelvis; the peritonitis radiating from the pedicle, not from the wound in the abdominal wall, nor from a surface where the cyst had been adherent." Mr. Wells says, in reference to the above case, that "the trials which I had made of returning the pedicle seemed to teach that in young or healthy subjects, where circumscribed peritonitis and effusion of plastic lymph might be expected, the practice was a good one; but in debilitated or cachectic patients, in whom diffuse peritonitis and effusion of serum, or of a plastic lymph, might be feared, it would be safer (where the clamp could not be used) *to leave the ends of the ligatures hanging out* through the wound, and thus *secure an opening for the escape of effused serum*, and for the ligature itself with the tissues enclosed in it after their separation."

No. 33 (Case XCVI.).—Operation, May, 1864. Death in forty-four hours. "On the morning of the second day after operation, the pulse was 150, and there was a little vomiting. Stimulants were freely given, but she died at 10 P. M., forty-four hours after the operation. No *post-mortem* examination was permitted." Mr. Wells assigns no cause of death.

No. 34 (Case XCVIII.).—Operation, May, 1864. Death in sixty-seven hours. At 11 P. M., pulse 110. Vomiting at 6 P. M. next day, with pulse 120. Vomiting continued, and pulse rose to 130; second day 5 P. M., pulse 140. Vomiting at intervals, 10 P. M., pulse 150. Lowest stitch removed, and the finger introduced into the abdominal cavity, *when about an ounce of bloody serum came away*. At 11.45 bandage removed, and effort made to pump out the fluid in the peritoneal cavity. Only two drachms were obtained. Pulse 150. Died sixty-seven hours after operation.

Post-mortem examination made by Dr. Ritchie, thirty-two hours after death. "*About a pint and a half of bloody serum without clot lay in the cavity of the pelvis.*" Mr. Wells says she died of septicæmia.

No. 35 (Case CII.).—Operation, July, 1864. Death in ninety-two hours. The patient went on badly from the first, pulse varying from 140 to 168. On second day pulse 160 all day, and "on the next day *there was a free oozing, partly of blood, and partly of reddish serum from the upper part of the wound.* After this she felt easier, the urine became more copious, and she took some nourishment, though vomiting was still troublesome. In the evening diarrhœa came on and some tympanites. On the third day, sixty-eight hours after the operation, I removed all the sutures, as the wound seemed to be firmly united, and I reapplied strapping. The pulse was then 168, and she complained of faintness and exhaustion. In the evening some bleeding came on from the lower part of the wound, and Dr. Parsons, finding it gaping, reapplied some of the sutures. She died ninety-two hours after operation. *Post-mortem* examination seven hours after death by Dr. Barratt."

"The sutures which were retained do not close the wound throughout, so that, at the central part, intestine can be seen clearly between edges—no union had taken place. . . . A diffused, dark, bloody discoloration of great omentum and surface of bowels; venous coagula and general infiltration around the right inguinal region; *seven ounces by weight of dark fluid blood were removed from the cavity of the pelvis*, and in some of the more dependent parts of the abdominal cavity there were small coagula. The right broad ligament of uterus had coagula adherent, and was reduced to a very short pedicle. The tumor must have been sessile almost. The pedicle has two transfixing ligatures which enclosed both the Fallopian tube and the round ligament; but a ligature which had been passed behind them at the time of the operation had evidently slipped off, and permitted oozing of blood from the spermatic veins. Some slight adhesion between neighboring folds of intestine were just commencing; but no general or partial peritonitis. . . . A tough fibrinous clot, free from blood-corpuscles, filled right ventricle with tenaciously-interlaced roots among the carneæ columnæ."

Mr. Wells says, "I looked upon the case from the moment of operation as one of clot in the heart."

No. 36 (Case CIII.).—Operation, July, 1864. Death on twenty-ninth day. Forty-seven hours after operation she was doing apparently well, and Mr. Wells removed all the stitches, as the wound seemed firmly united. At 9 P. M., on the third day, after an attack of vomiting, the lower part of the wound opened, and *a good deal of reddish serum escaped.* On fourth day, a full inch of the wound was open, but no intestine to be seen. Mr. Wells closed this by two hare-lip pins, leaving the *lower angle open to admit the escape of serum.* Fifth day, very low, pulse 140, tympanitic. Abdomen evidently containing fluid. Examination detected fluctuation high up behind the uterus. Vomiting with prostration continued, and on the sixth day (pulse 160) one hare-lip pin was withdrawn, and one finger passed into the peritoneal cavity, but *only about half an ounce of reddish grumous fluid came away.* At midnight the wound commenced discharging, and then the patient went on favorably.

On the seventh day, on inquiry into the history of this favorable change, it appeared that, in consequence of some lumps of fæces having been discovered in the rectum, an enema of soap-and-water had been ordered. A good deal of fecal matter had been evacuated, and (perhaps from the exertion of getting on the bed-pan) *the wound had discharged about three ounces of pinkish-white, thick, curdy fluid;* shortly after this the patient slept soundly for two hours, the pulse having fallen from 160 to 136.

On the eighth day *a free fetid discharge was coming from the wound.*

On the twelfth day passed a trocar into Douglas's *cul-de-sac, and three and a half pints of abominably fetid, black, tarry fluid were evacuated.*

On the sixteenth day the trocar was again introduced into the recto-vaginal pouch, and rather more than a pint of black, fetid fluid discharged. Next day canula replaced, and some more fluid discharged. The fluid drained all night, about a pint and a half having been passed since the reintroduction of the canula. On the twenty-fist day the discharge was free, fetid, yellow, and purulent, pulse 140. On the twenty-fifth day the same state, the fetid fluid still trickling away from the tube. The canula was withdrawn, and the flow ceased on the next day. By probing the cavity, one ounce of fluid came away. Death took place on the twenty-ninth day after the operation.

Post-mortem examination twenty hours after death by Dr. Ritchie and Mr. McFarlane.

The place of the left ovary was occupied by a little capsule of lymph enclosing the ligatured stump. The recto-vaginal pouch was empty. The utero-vesical pouch was filled with creamy pus. The large, irregular cavity behind the uterus was capable of holding about a gallon of fluid; during life it must have been full of air. It was bounded superiorly by coils of small intestines, firmly adherent to each other and to the anterior abdominal wall. Pus was found incarcerated in spaces between the coils of intestine, thus forming a large number of circumscribed abscesses. Died of *pyæmia.*

No. 37 (Case CVII.).—Operation, October, 1864. Death on the eleventh day. A large cyst had burst eight days before the operation, and the peritoneal cavity was filled with a clear, amber-colored, tenacious fluid, thick as calves-foot jelly. The pedicle was secured by a ligature and chain of *écraseur.* The pedicle seemed to be friable and there was some danger of its giving way. On the third day the chain and ligature separated from the pedicle, but there was no bleeding.

Two of the stitches were removed forty-eight hours after the operation. Tympanites was then getting troublesome. Three days after the operation *a gush of reddish serum escaped beside the pedicle.* On the fourth day there was vomiting, and Mr. Carden wrote to Mr. Wells that "the abdomen was enormously distended, the ligatures scarcely holding the wound together. A ragged, sloughy shred of pedicle lay at the bottom of the incision. Vomiting and hiccough. On the seventh day vomiting recurred in the afternoon, and, after a severe straining, *about a quart of pale, clear serum gushed from beside the remains of the pedicle.* She died on the eleventh day."

Mr. Wells says: "Unfortunately, no *post-mortem* examination was permitted; but it seems pretty certain that the peritonitis which had been set up before the operation was never subdued, and latterly the ordinary effects of peritonitis were complicated by septicæmia, some of the fetid fluid from the surface of the pedicle in all probability having been absorbed."

No. 38 (Case CXIII.).—Operation, November, 1864.

Death in sixty-seven hours. On the day after the operation, "at 11 A. M., after some coughing and retching, the clamp came off, and *there was a little serous discharge beside the pedicle.* In the afternoon I found that the pedicle had sunk partly into the abdomen, and was almost out of sight, although some shreds adhered to the edges of the wound. There was no bleeding; she vomited all the afternoon. The pulse was 140, and she died at 10 A. M. on the third day, sixty-seven hours after the operation."

Mr. Wells assigns no cause of death.

No. 39 (CASE CXV.).—Operation, January, 1863. Death on the seventh day. Patient did well for forty-eight hours. Pulse 100. (After cutting away the tumor there was some oozing of blood around the clamp, but it was stopped by tying a ligature tightly around the pedicle beneath the clamp.) "I removed the clamp forty-four hours after the operation, as it seemed to be lying quite loose on the wound; the ligature which had been tied beneath it also came away with a shred of dead fibrous tissue. There was no bleeding. I also removed three of the sutures. On the third day after the operation, there was some flatulent distention of the abdomen and frequent eructation, but no vomiting. The rectum was cleared by an enema. At 9 P. M., during one of the 'fits of belching' as the nurse called them, *the lower part of the wound gave way, and a knuckle of intestine protruded; a good deal of fetid serum also escaped.* I returned the intestine, reapplied three sutures deeply, and the patient did not seem to be worse. On the next day there was free fetid discharge from the lower part of the wound, and vomiting became troublesome." Tympanites increased, she became weaker, and died on the seventh day, or one hundred and fifty-four hours after the operation.

Post-mortem examination: "*There was a good deal of fetid serum in the peritoneal cavity, and some traces of recent peritonitis were also shown by flakes of lymph.* There was no blood nor clots to be seen, and only one or two shreds of sloughy tissue at the spot where the tumor had been removed from the right side of the uterus. The pedicle of the tumor first removed connected the left side of the uterus closely with the abdominal wall."

Mr. Wells assigns no cause of death in this case.

Having now gone perhaps too minutely into the pathological appearances found in Mr. Wells's thirty-nine fatal cases, I have arranged them in the following table; the first column designating the causes of death according to Mr. Wells's views; the second according to my own interpretation of the symptoms or pathological appearances observed in each case. They are placed in the order in which they stand in Mr. Wells's book and in the preceding pages.

In many cases Mr. Wells has omitted to state positively the cause of death; in some of these I have inferred from his language what he supposed the probable cause to be, and I have so put it down; while in others, where I had nothing upon which to base my conclusions, I have placed them under the head of "not stated." Rigorously, this class should have been much larger than it is.

Causes of Death, according to

No.	Case.	Wells.	Sims.
1....	IV	Not stated	Septicæmia.
2....	VI	Peritonitis	do.
3....	X	do.	do.
4....	XII	Tetanus	Tetanus.
5....	XIV	Peritonitis	Septicæmia.
6....	XVI	Septicæmia	do.
7....	XVII	Intestinal obstruction	do.
8....	XXIV	Septicæmia	do.
9....	XXV	Exhaustion	do.
10....	XXVI	do.	do.
11....	XXIX	Peritonitis	do.
12....	XXXI	do.	Pyæmia.
13....	XXXII	do.	Septicæmia.
14....	XXXIII	Exhaustion	do.
15....	XXXIV	Septicæmia	do.
16....	XXXV	Tetanus	Tetanus.
17....	XLV	Peritonitis	Septicæmia.
18....	LIV	do.	do.
19....	LIX	Pyæmia	Pyæmia.
20....	LX	Exhaustion	Septicæmia.
21....	LXV	Heart-clot	do.
22....	LXVII	Exhaustion	do.
23....	LXXI	Heart-clot	do.
24....	LXXII	Exhaustion	do.

Causes of Death (continued) according to

No.	Case.	Wells.	Sims.
25	LXXIV	Peritonitis	Septicæmia.
26	LXXV	do.	do.
27	LXXVIII	Septicæmia	do.
28	LXXXI	do.	do.
29	LXXXII	Not stated	do.
30	LXXXIII	do.	do.
31	LXXXIX	Peritonitis	do.
32	XCII	Exhaustion	do.
33	XCVI	Not stated	do.
34	XCVII	Septicæmia	do.
35	CII	Heart-clot	do.
36	CIII	Pyæmia	Pyæmia.
37	CVII	Septicæmia	Septicæmia.
38	CXIII	Not stated	do.
39	CXV	Peritonitis	do.

Now, let us sum up these two columns and see how the causes of death in these thirty-nine cases have been interpreted by Mr. Wells and myself:

Wells.		Sims.	
Intestinal obstruction	1	Tetanus	2
Tetanus	2	Pyæmia	3
Pyæmia	2	Septicæmia	34
Heart-clot	3		
Exhaustion	7		
Peritonitis	12		
Septicæmia	7		
Not stated	5		
	39		39

Thus it will seen that, the two cases of tetanus excepted, I make all the deaths the result of blood-poisoning, three of pyæmia and thirty-four of septicæmia. Whenever in the progress of a case, after operation, there was a discharge of reddish serum beside the pedicle, I put it down as septicæmia, even when there was no *post-mortem* examination. I have assumed that others died of septicæmia, because the symptoms and manner of termination were such as to justify in my mind this conclusion. But let us see what interpretation may be put on these thirty-nine cases, by excluding all in which there were no *post-mortem* examinations. In eleven out of the thirty-nine there were no *post mortems.* To these let us add the two tetanic cases, and that leaves us twenty-six to be dealt with in a strictly pathological sense. By running over these as epitomized in the preceding pages, I find that the peritoneal or pelvic cavity contained—

Acrimonious fluid in	1
Fetid serum	1
Clear serum	3
Turbid serum	3
Reddish serum	16
Pus	2
	26

Thus it will be seen that, according to the pathological appearances as noted by Mr. Wells, two had pyæmic poisoning, and twenty-four presented the infallible *post-obit* evidences of septicæmia. There cannot be the slightest doubt about any of these items except the clear serum. It then becomes a question of what is clear serum. One of these cases is noted as such after the characteristic *reddish serum* had been accidentally forced out beside the pedicle on the second day after the operation, and hence I have a right to suppose there was some error or rather omission of the term reddish or sanguinolent in the notes furnished Mr. Wells.

In another, Mr. Wells himself supposes that the fluid found was clear ovarian fluid, which had been left in the bottom of the Douglas *cul-de-sac.*

I think the absolute pathological appearances found in the twenty-six cases fully justify the conclusions that I have drawn from the symptomatology of the eleven non-*post-mortemized* cases.

In every one of my seven *post-mortem* examinations, and in every one of Mr. Spencer Wells's twenty-six, we find uniformly the same pathological appearances. In all of them we find a quantity of reddish serum, or grayish turbid serum, or fetid serum, or putrid serum, or acrimonious serum, in the peritoneal cavity; and in cases slowly dying of pyæmia we invariably find pyogenic reservoirs in the pelvic cavity. Is it not logical to infer that these pent-up fluids are the causes of the blood-poisoning that so uniformly, I should say universally, attends fatal cases of ovariotomy?

I do not pretend to deny that death may occur from shock, or from hæmorrhage, or from heart-clot, or from exhaustion, or even from peritonitis; but I feel sure that these, independently of blood-poisoning and its legitimate causes, are of comparatively rare occurrence. If, then, we have such an almost universal evil to deal with as septicæmia, and if that septicæmia is, in thirty-seven cases out of thirty-nine, clearly traceable to the poisonous fluids effused in the peritoneal cavity, is it not self-evident that the indication, both of prevention and cure, is to drain off these poisonous fluids in the speediest and most direct way possible? My facts and premises granted, the conclusion is inevitable—and we must all say, yes. But many will reply that this is nothing new, that it has been often done before, and many lives have already been saved by it.

All this is true. The great McDowell had a drain from the peritoneal cavity, when he left the ligature of the pedicle hanging from the external wound. Clay does the same thing in the same way to-day, and for the same purpose. Peaslee does it when he rolls a tent the size of the index-finger, and leaves it in the lower angle of the abdominal incision, to allow the peritoneal cavity to be washed out if necessary. Spencer Wells has had the abdominal wound to accidentally burst open, and allow the escape of pent-up reddish serum. Kœberlé has intentionally opened the cavity at top and bottom for the evacuation of fluids. Keith and Kimball and others have done the same thing.

Few men have seen much of ovariotomy, who have not seen cases where a favorable result was obtained by drainage, and ablution of the peritoneal cavity. And yet no one has formulated the principle into a law. The cases in which it has been done were either accidental or exceptional.

As said before, Mr. Spencer Wells has had several cases of recovery from septicæmia, by opening the peritoneal cavity in the line of abdominal incision, and allowing the reddish sero-sanguinolent fluid to run out; and he has had some cases of pyæmia cured by puncturing pouches of putrid fluid through the *cul-de-sac* of the vagina. In the first, the peritoneal collections have been generally accidentally forced out beside the pedicle, by the tympanites and forcible vomiting.

In the second, he detected swellings in the *cul-de-sac* of the vagina, and punctured with a trocar.

In May, 1862, Mr. Wells removed a multilocular cyst (Case XXXVI., page 107), in the presence of Mr. Nélaton, Dr. Ramsbotham, Dr. Protheroe Smith, and others. The patient went on "fairly well," the pulse varying from 110 to 120 for ten days, when she began to look yellow, to lose appetite, and to feel very weak, and there was a discharge of gelatinous mucus from the rectum. On examination, Mr. Wells found a depression of the recto-vaginal septum, which led him to suspect the presence of some serous or purulent fluid in the pelvic cavity. On the twelfth day she was much worse, and Mr. Wells, finding the recto-vaginal pouch more prominent, passed a trocar into it just behind the womb, and evacuated "eight ounces of very fetid bloody serum; which was followed by a discharge of grumous pus, and by immediate relief, with improvement in her general condition. Two days afterward a very free discharge of fetid pus escaped by the side of the remains of the pedicle; and this continued in varying quantities for several days; but she gradually improved," and left the hospital a month afterward in good health and spirits.

Curiously enough, on the day that the above case was discharged from the Samaritan Hospital, Mr. Wells operated on another patient (Case XXXIX.), whose case presented the same peculiarities and had the same fortunate termination as the one just narrated. For some days after the operation the pulse was rapid; the face dusky-jaundiced; the urine ammoniacal, and a pelvic abscess was suspected. About ten days after the operation he discovered a fulness between the uterus and rectum, but he did not feel warranted in puncturing it till, in three or four days more, there were such unmistakable signs of a pelvic collection that he passed a trocar into the swelling behind the uterus and evacuated "eight ounces of dark-colored fœtid fluid. She said she felt immediate relief, and she continued to improve from that time."

In Mr. Wells's Case LIX., a synopsis of which may be found on page 575, he tapped the pelvic gathering through the Douglas *cul-de-sac*, but the patient eventually died from pyæmia, twenty-six days after the operation. The true nature of the complication had not manifested itself early enough to call for the only means that held out the slightest hope of saving life.

In November, 1863, Mr. Wells lost a patient (Case LXXXI.), on the seventh day after operation, with fully-marked symptoms of septicæmia, and on *post-mortem* examination he found "*about a pint of turbid serum* in the pouch of the peritonæum between the uterus and rectum." Mr. Wells remarked to the gentlemen at the *post-mortem* examination: "If I had made a puncture by the vagina and let off the fluid, as I had done in three other cases, I think the patient would have had a much greater chance of recovery; but nothing had led me to suspect the presence of the fluid, or to make the examination by which it would have been detected. The case shows the importance of making vaginal examinations when bad symptoms come on after ovariotomy."

Mr. Wells's fourth case of puncturing through the Douglas *cul-de-sac*, for the evacuation of pent-up fluids, is on page 261 of his book (Case CI.). This patient did well till the ninth day, when she was found in a typhoid state, with dry tongue, dilated pupils, flushed face, and drowsiness. As she continued to grow worse, Mr. Wells made a vaginal examination in the afternoon, and, detecting fluid, "made a puncture by a trocar and let out five ounces of dark, bloody serum, which had a putrid, ammoniacal odor. This was followed by some relief. The pulse sank from 112 to 95 and 92, but mucous diarrhœa came on, and the typhoid condition was aggravated next day. As the discharge from the trocar puncture had ceased, an examination detected fluid still in the recto-vaginal space." Mr. Wells made another opening into this space, evacuated ten ounces of fluid still more putrid than that of the day before, and containing pus. He then carried on the trocar through the opening made the day before, and drew a drainage-tube through the canula, before withdrawing the latter. The tube was then tied and left fixed. He took great care that it should pass through the lowest point where the peritonæum is reflected from the rectum to the vagina. Very free discharge came through the tube for several days, and the general condition rapidly improved. The tube was removed at the end of about nine days, and she left in a fortnight, and regained her health perfectly.

Mr. Wells, in remarking upon this case, said that "the danger of puncture had been very greatly exaggerated; that the benefit of the evacuation of fluid is often very marked; and that any danger arises from too early closing the opening, not from the opening having been made. Where, in cases of blood-poisoning, it is very important to maintain a free passage for putrid fluids, the drainage-tubes of Chassaignac render most valuable service."

The last case, illustrating this subject, that I shall quote from Mr. Wells, was operated upon in July, 1864 (Case CIII., page 271), and is here epitomized on page 582. She was very ill after the operation, and on the third day the wound in the abdomen gave way, and "a good deal of reddish serum" escaped from its lower angle. The case progressed badly, and, about a fortnight after the operation, Mr. Wells found fluctuation in the Douglas *cul-de-sac*, and with a trocar "three and a half pints of abominably fetid, black, tarry fluid were evacuated." There was more or less discharge for about twelve days longer, the patient, after a hard struggle for life, at last dying of pyæmia.

It will be perceived that, in all the cases quoted from Mr. Wells, the punctures were made not into the peritoneal cavity, properly speaking, but into distinct sacs containing the poisonous fluids. In all these cases there is every probability that the punctures were made into pouches isolated or shut off from the peritoneal cavity. In those that died, the *post-mortem* examinations proved this to be true. In Case LXXXI. (page 203), where death from septicæmia occurred on the seventh day, the *post-mortem* examination revealed the fact that "the fundus uteri lay on a level between the symphysis pubis and promontory of the sacrum. A layer of recent gelatinous lymph formed a sort of arched roof from the sacrum and rectum over the uterus to the bladder and pubes, enclosing about a pint of turbid serum in the pouch of peritonæum between the uterus and rectum, and to the right side of the uterus. The pouch was lined with a layer of lymph."

These cases of Mr. Wells were plainly examples of pelvic abscess, with consequent pyæmic poisoning; and there could not have been the slightest doubt about the propriety of the course of treatment to be pursued. But what I am more particularly interested in is septicæmia from poisonous fluids, whether sanguinolent serum or turbid serum in the true cavity of the peritonæum.

Dr. Thomas[1] says: "The most valuable suggestion with reference to this matter has emanated from Dr. Peaslee, who has unquestionably placed at the disposal of the ovariotomist a method which robs the operation of much of its danger. It consists in washing out the peritonæum with disinfectants." Now, let us see what Dr. Peaslee has done, and what he has to say on this point.

Dr. Peaslee[2] says he first used intra-peritoneal injections in February, 1855, and considers them "more valuable and effectual than all other methods" in treatment of septicæmia after ovariotomy. He cites the following cases:

Case I.—Septicæmia from decomposed ascitic fluid; cured by intra-peritoneal injections; an ovarium tumor complicated with ascites removed, February, 1855, one hundred and six pounds of ascitic fluid had been withdrawn by a previous tapping. It was feared that the immensely distended peritonæum would continue to secrete the ascitic fluid after the operation, and a gum-elastic catheter was passed by the vagina, behind the uterus, through a puncture into the Douglas *cul-de-sac* and there left. On the sixth day symptoms of septicæmia appeared suddenly, and on removing the cork which had been placed in the catheter a small amount of very fetid fluid escaped. A quart of pure water (98° Fahr.) was at once injected through the tube into the peritoneal cavity and then allowed to flow out again. This was repeated the second time, when the patient said: "I feel refreshed, as if I had taken a bath." She then became bright and natural, but the septicæmic symptoms would return in from eight to twelve hours, and the injections were repeated. Afterward a solution of salt, ʒj (to ʒij) to Oij water was used, and later a solution of liq. sodæ chlorinatæ ʒj to ʒij to Oj water was alternated with the salt-and-water. These injections were used two or three times daily for seven days, when the odor of decomposition was gone, the tube withdrawn, and the patient had a favorable recovery.

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[1] "Diseases of Women," third edition, p. 751.

[2] "Ovarian Tumors," Peaslee, p. 509.

Case II.—Same class as preceding; cured by injections. At first a warm, aqueous solution of salt and albumen was used in order to imitate the natural secretion of the peritonæum. But the albumen was decomposed by commixture with the fluid in the cavity, and the solutions described above were resumed. The injections were used twice daily for a time, and then once a day for fifty-nine days in all. The fetor then ceased, and the patient recovered. In this case the symptoms of septicæmia did not occur until the eighteenth day.

Case III.—Septicæmia produced by blood oozing from vessels of the omentum, and subsequently continued by pus forming on a granulating surface; both fluids were decomposed in the peritoneal cavity—cured by injections. The symptoms from the decomposed blood appeared on the fourth day, but were not decided until the seventh, when the injections were commenced. Sometimes ʒj of the liq. sodæ chlorinatæ to ℥iv of water was used; after all the blood was removed the formation of pus necessitated three daily injections for twenty days in order to keep the patient from sinking; then two a day for twenty-one days; then one daily for thirty-three days; making, *in toto*, one hundred and thirty-five injections in seventy-eight days! In the preceding case the injections were used four weeks, and in this case three weeks, before the least improvement in the character of the fluid could be perceived.

Case IV.—Septicæmia from blood-clot. Injections commenced on the twelfth day; death on the seventeenth day. The blood-clot (probably about four ounces at first) was from some small omental vessels which did not bleed during the operation, or for twenty minutes after it, as the incision was kept open for that time to give an opportunity for oozing blood. The septicæmia was declared the twelfth day, and the injections were commenced. They always returned from the cavity the color of dark beef-broth, with oil on the surface, though used three times daily. The patient died on the seventeenth day, and the autopsy showed a decomposed clot, about three times the size of a hen's-egg, between the posterior surface of the omentum and the small intestines. The injections had operated on this mass too slowly to save life. Had the symptoms warranting them occurred several days sooner, the result might probably have been different. Dr. Peaslee says that, of these four patients, "three had unquestionably been rescued from death" by the injections, a result which he believes could have been obtained in no other manner.

He also notes with interest how quickly the blood threw off the septicæmic agent it contained, by the use of the injection, although it was again rapidly poisoned by reaccumulation of the fluid. Feeling that in these cases he had only *removed* septicæmia, he now resolved to attempt its *prevention*, and that in every case where he had any reason to fear that blood, ascitic fluid, pus or fluid from the ovarian tumor, could remain or collect in the peritoneal cavity after the operation, he would leave a tent in the lower end of the incision, which should remain undisturbed until the fourth day, unless symptoms demanded its removal sooner. When removed, the cavity should be examined for fluid, and the opening closed or the injections used as the examination should decide. He never used the glass tube as employed by Kœberlé, as he considers that the use of any tube invites septicæmia.

Case V.—Septicæmia feared; no fluid found in peritoneal cavity; recovered. A large fibro-cystic tumor of the uterus, with very extensive adhesions, was removed, and the peritoneal cavity well sponged out. Hæmorrhage was feared from the lacerated surfaces, and a tent of moistened and firmly-twisted linen was left between the two lowest of the silver sutures which closed the incision. This tent was three-eighths of an inch in diameter, and projected half an inch into the peritoneal cavity. After ninety-six hours no symptom of blood in the peritoneal cavity appeared, and the tent was removed, and a silver prostatic catheter was passed through the opening to the bottom of the pelvis, where alone the fluid would be found if it existed in very small quantity. The instrument was introduced with the index-finger applied air-tight to its distal extremity; when the beak reached the floor of the pelvis the finger was removed, to allow any fluid there to enter it, and replaced before the instrument was withdrawn, in order to retain the fluid, though only a drop or two. The instrument being withdrawn, the beak was placed on clean white paper and the finger removed, when the color and properties of the fluid could be perfectly perceived. This proved a very satisfactory test of the amount and quality of the fluid in the peritoneal cavity. But two or three drops of serum, faintly tinged with blood, of natural odor, were obtained. The tent was not replaced, the incision was finally closed, and the patient recovered.

Case VI.—Threatened septicæmia from internal hæmorrhage prevented by the injections; recovery. In a case where, like the former, hæmorrhage was feared from the detachment of very vascular adhesions, a tent was left in the lower part of the incision; forty-eight hours after the operation, the tent was saturated with blood; on its removal, about three gills of serum, deeply tinged with blood, and some gas, not fetid, issued from the peritoneal cavity. Dr. Terriberry, of Paterson, New Jersey, had the after-care of this case, and he at once commenced the injections of salt-water with carbolic acid (gr. j to Oj), and used them twice daily for twenty-nine days, and once a day for fourteen days more. The freshness of the blood discharged showed that the hæmorrhage continued thirteen days after the tent was removed, and fifteen days after the operation. On the ninth day, there was evidence of a clot in the pelvis, and, remembering Case IV., Dr. Peaslee advised Dr. Terriberry to break it up by the gum-elastic catheter used for the injections. This being done, fresh blood and clots were discharged for six days; then clots for seven more; then two or three large, black coagula came away, and the fluid became purulent and less in quantity, until it ceased on the forty-third day. A peculiar fact in this case was the tendency of the opening to close. After the third week it was often necessary to insert a sponge-tent. There were no symptoms of septicæmia at any time, and never more than a slight odor of decomposition. The patient was reduced by the continued hæmorrhage, but had a good recovery.

Case VII.—Septicæmia threatened from colloid contents of ovarian cyst retained in the peritoneal cavity, but prevented by the intra-peritoneal injections. In spite of thorough sponging, some portions of the viscid, colloid contents of a polycystic ovarian tumor, which had been removed, adhered to the peritonæum. A tent was used as before. After ninety-six hours, no bad symptoms arising, the tent was removed, when a quart of very offensive, light-brown fluid issued from the peritoneal cavity. Three quarts of warm salt-water (ʒj salt to Oj water), with three grains of carbolic acid, were injected, and repeated twice more, until the fluid returned with no odor of decomposition. This injection was repeated each afternoon for three weeks, four grains of carbolic acid to a quart of water being used at last. After the commencement of the third week the fluid was also pressed out of the peritoneal cavity daily, at six o'clock, A. M. The third day the injections were used, about ℥viij of pure pus were obtained; this diminished gradually, and, at the end of the third week, amounted to ʒj in twenty-four hours; the fourth week but two injections were used, and the pus was less than ʒj daily. At the end of six weeks the pus amounted to a mere trace, and the injections were abandoned. The patient recovered, but died, five months after the operation, of embolism of the left middle cerebral artery. It is worthy of note that, at the end of four months and a half, the opening had not closed, and a few drops of pus issued daily. A small gum-elastic catheter was then passed into the track taken by the pus, and a small quantity of salt water injected; the second day after, the ligatures of the pedicle were thrown off.

In a review of these cases by Dr. Peaslee we find that, of four cases of developed septicæmia, three were cured by intra-peritoneal injections, and one died. The internal remedies doubtless sustained the first three, and to some extent counteracted the septic influence, but, had the decomposed fluid not been removed, death must have resulted in every case. In the cases where septicæmia was feared, it was *undoubtedly prevented* by the injections. Dr. Peaslee adds that, of these seven cases, all, except number five, were in a condition much below the average as candidates for ovariotomy. He therefore concludes that—

1. Intra-peritoneal injections of water at 98°, with the addition of liquor sodæ chlorinatæ, or carbolic acid, as before explained, are entirely safe after ovariotomy, in the conditions requiring them.

2. They should be used with a *curative* intention in all cases of septicæmia already developed, and in all cases for *prevention*, where it is feared from the presence of any fluid in the peritoneal cavity, whose decomposition will produce it.

3. Thus used, they may reduce the percentage of deaths from septicæmia after ovariotomy, from one-sixth of all who die after it, to one thirty-sixth, and increase the average success of ovariotomy four or five per cent.

4. Intra-peritoneal injections are never to be thought of except for the purpose of removing a fluid already in the peritoneal cavity, which either already has, or assuredly will have, produced septicæmia.

5. A tent may be inserted for two to four days at the lower end of the incision, with entire safety, in any case of ovariotomy where the accumulation of such fluid is apprehended.

6. Finally, septicæmia would more rarely occur after ovariotomy if all fluid were removed from the peritoneal cavity by the most careful sponging before closing the incision.

In these liberal quotations from Dr. Peaslee I have tried to do him justice, and to give him full credit for the originality and success of his practice. To him we owe the principle of intra-peritoneal medication. I claim only to be his coadjutor. But, the rule that he would establish for exceptional cases, I wish to make applicable to all alike; and, instead of washing out the peritoneal cavity at the top, I propose to open it at the bottom, as he did in 1855, and let the fluids run out spontaneously and continuously. The natural outlet of the peritoneal cavity is through the Douglas *cul-de-sac*.

If the drainage through this *cul-de-sac* be made promptly and thoroughly, there is every reason to believe that pyæmic collections, such as happened to Mr. Spencer Wells, could never take place; and that septicæmic poisoning would in a great measure be prevented.

What I propose, then, is simply this: to puncture the *cul-*

de-sac of the vagina behind the cervix uteri, and to pass a tube of some sort into the peritoneal cavity to drain off any effusion that may take place in said cavity. I propose to do it in every instance, whether there are adhesions or not. Patients may and do die of septicæmia after ovariotomy, where there are no adhesions. Hence it is safe to make this puncture in every case, whether simple or complicated. If in three or four days we see that there is no necessity for this precautionary step in the operation, we have nothing to do but to remove the tube, and in twenty-four hours the little puncture closes up spontaneously. Where is the harm, then, of making the puncture? It adds nothing, not the prick of a pin, to the danger of ovariotomy. It cannot, possibly, do the least harm, and it may be the means of saving life. It may be said that it is time enough to make the puncture when bad symptoms arise. But we know that many cases go on very well for two or three days, when, all at once, there is an explosion as it were; the pulse suddenly jumps from 100 to 120, and then to 140, with vomiting, tympanites, hot skin, and oppressed breathing; the pulse gets quicker and more feeble, and the patient sinks exhausted in sixty or seventy hours after operation. We are in doubt what else to do but to give stimulants; and it is too late to be operating in the dark on a dying patient's vaginal *cul-de-sac.* During the ovariotomy it would have been nothing to puncture it, but now it becomes a difficult thing to do, and we naturally shrink from tormenting one in such extreme agony and prostration, unless we should feel sure of finding the cause of the evil, and of warding off the dangers of the moment. But in my own mind I am sure of what generates these alarming and too often dangerous symptoms, and I beg to urge upon my brethren, with all the earnestness of a deep-settled conviction, the propriety, the necessity, the safety, and the efficiency, of resorting to this drainage by the vagina in every case of ovariotomy, both as a preventive and curative agent against septicæmia, which, I am sure, is the great danger in this formidable operation. For many years I have been convinced that a puncture of the *cul-de-sac* of the vagina would be a sure means of obviating some of the dangers of this operation, and in the spring of 1864, when I assisted Mr.

Nélaton and Sir Joseph Oliffe in an ovariotomy, and when, twenty-four hours afterward, I saw that the patient must die, I said to Mr. Nélaton that she was dying from an effusion of a reddish serous fluid in the peritoneal cavity, and I suggested to him then to puncture the *cul-de-sac* of the vagina, and let it run out. It was not done. The patient died, and I assisted Mr. Nélaton and Sir Joseph Oliffe at the *post-mortem* examination, when we found precisely what I had predicted; viz., a large effusion of a sero-sanguinolent fluid (a quart or more) completely filling the pelvic and abdominal cavities. Mr. Nélaton called it peritonitis with aplastic effusion. Until a recent date, I was under the impression that it would do to wait for dangerous symptoms to arise before making the puncture, but on this point I have changed my mind, and I believe the puncture should be made at once, and in every instance.

In May (1872) I operated, at the Woman's Hospital, on an English lady, aged forty-six, the mother of a grown-up family of children. Her health had always been good. The tumor was of about three years' standing. It was enormous. She measured, around the largest part of the abdomen, fifty-nine inches, and, added to this, she had a very large umbilical hernia, certainly as large as the two fists. Dr. Emmet had seen the case a year before, but, as her health was then good, and she did not suffer particularly, he wisely put her off till urgent symptoms should demand surgical interference. On examination, I pronounced it a case of unilocular ovarian cyst. Dr. Thomas, Dr. Peaslee, and Dr. Emmet, all thought the operation called for; but Dr. Emmet, who had seen the case in its earlier stages, had some misgivings about the relations of the tumor and the uterus. The operation was performed with the assistance of Drs. Emmet and Thomas, and the staff of the hospital. There was nothing peculiar in the operation till we came to its conclusion. The unilocular cyst was without adhesions anteriorly, or to the viscera of the abdominal cavity, but it adhered so intimately to the posterior surface of the body of the uterus, and to the left broad ligament, that we were obliged to follow Dr. Miner's plan of enucleation. We accordingly enucleated the sac entirely out of its peritoneal capsule, and, instead of having a solid pedicle to deal with, we had this hollow sac, which we gathered into folds, and drew out through the lower end of the external incision, and fixed it with a clamp, just as if it were an ordinary pedicle. The bleeding from the cavity of this sac, from which the tumor had been rolled or skinned out, was very profuse, and it was necessary to use the liq. fer. subsulph. freely, and even to apply it on a tent of cloth, which was subsequently removed. Besides this tumor there was another, about the size of a fœtal head, occupying the place of the right ovary, with the same sort of sessile attachments to the right broad ligament. This being, like the other, without a pedicle, was likewise enucleated, and its membranous sheath was gathered into folds and pulled out through the external incision, and clamped as the other was. We had nearly as great difficulty in controlling the hæmorrhage in this as in the first one. Not expecting to find any attachments or any abnormal condition of things, I was not prepared for the emergency. I felt convinced at the time that I ought to have made a double puncture through the vagina, one in the bottom of each of these sacculated peduncles for drainage-tubes, but, having no facilities at hand for this purpose, I thought it would be enough to leave the abdominal incision open at its lower end; and I determined to pass a trocar down through each sac to the bottom of the pelvis, and out into the vagina, through the Douglas *cul-de-sac* for drainage, whenever the symptoms should become urgent. There was considerable reaction after the operation, and my patient seemed to be strong, and I hoped she would be able to throw off the disease. I went to the hospital every day, prepared to puncture and pass drainage-tubes, but I did not think her in so dangerous a condition as she really was, and she died on the third day, when I was by no means expecting such an early termination of the case. A *post-mortem* examination showed each one of the sacs, from which the tumors had been enucleated, filled with a dirty-grayish turbid serum; not more than four ounces in the larger, and two in the smaller one; no peritonitis, but there was a large effusion of venous blood in the cellular tissue behind the peritonæum, reaching from the lower part ot the pelvis up toward the right kidney. This coagulum had, of course, nothing to do with the fatal result; she died of septicæmia—a termination that might have been averted if I had done what I ought to have done, and what I must implore all others to do, under like circumstances; viz., to establish a free and thorough drainage through the vagina at the moment of the operation.

This case was a sad lesson to me; for I felt that a valuable life had been sacrificed; and I then made up my mind to institute, in every instance thereafter, the drainage system at once, whether the case was simple or complex, adhesions or no adhesions.

Since then (May, 1872), I have performed four ovariotomies with the drainage plan.

Its utility may be inferred upon theoretical grounds, but can only be established by direct experiment. Although I have treated but four cases by drainage, my experience has been such that I feel justified in bringing the subject thus prominently before the profession, that others may be induced to adopt the same method.

For the notes of the following case I am indebted to Dr. O'Byrne, lately house-surgeon to the Woman's Hospital:

A maiden lady, aged sixty-two, was admitted to the Woman's Hospital, on the 26th June, 1872. Change of life occurred at forty, and she had enjoyed a good degree of health till about a year ago; when she took a long walk, became very much fatigued, and felt a severe pain in the left side, and for the first time detected a swelling there. She consulted her physician, who diagnosed ovarian disease. A week after this there was a flow from the uterus, resembling the menses, which lasted six weeks. About the same time her bladder became irritable, and she was obliged to pass water frequently. The tumor began to grow rapidly, and is now very large.

The following were the measurements on admission:

Circumference at umbilicus........................	40	inches.
From ensiform cartilage to umbilicus..............	11	"
From umbilicus to symphysis......................	10	"
" " to right ant. sup. spinal process.....	10¾	"
" " to left superior spinal process......	10½	"

The tumor has never been tapped, and has given her comparatively little inconvenience. She has no pain, but a constant sense of soreness over the abdomen, with tenderness on pressure. No vomiting, no constipation, eats pretty well, but is losing flesh very rapidly.

Diagnosis.—Multilocular ovarian tumor. It was a question whether the operation should be performed now or in the month of September, when the hospital would be reopened for the reception of patients.

Drs. Emmet, Thomas, and Peaslee, were called to see the case with Dr. Sims, and it was agreed that the patient stood a better chance of recovery if the operation should be done at once than if it were procrastinated. The patient was anxious to have it done immediately; and it was accordingly performed the next day (June 27), Dr. Sims being assisted by Drs. Emmet, Thomas, and Peaslee. An incision was made from the umbilicus nearly to the pubes. At the lower end of the incision, a large, tortuous vein, lying in or on the peritonæum, was cut, which bled furiously, and was secured with great difficulty, and only then by passing a loop of silver wire through the abdominal walls, just below the lower end of the incision, through the peritonæum, behind the wounded vein, and then out again through the parietes, where the two ends of the wire were tightly twisted, thus forcibly compressing the vein against the abdominal walls, and instantly stopping the bleeding. When the peritoneal cavity was opened, a large quantity of clear, albuminous-looking fluid escaped, supposed to be from a ruptured cyst of some portion of the tumor. As soon as this free fluid passed off, the shining sac of the tumor appeared at the incision, and the doctor passed his hand between the tumor and the walls of the abdomen, freely breaking up adhesions that were formed everywhere anteriorly. No little difficulty was experienced in performing this part of the operation, for the adhesions were numerous and firm. Spencer Wells's trocar was passed into the largest sac, and a dark, grumous, albuminous fluid was evacuated, and the more solid portion of the tumor was then drawn through the opening, when it was found closely adherent to the omentum and to the right broad ligament. These adhesions were separated, and the pedicle was transfixed by a silver wire, made to enclose it in a figure-of-8 loop, which was tightly twisted, cut off close, and the pedicle, being severed about three-fourths of an inch beyond the constricting wire, was dropped back into the pelvis. A large surface of the omentum which had been separated from the tumor continued to ooze blood pretty freely. After waiting some time for the bleeding to cease, it was determined to remove all that portion of omentum that was the source of trouble. It was about five inches by three. Its base was tied, in sections of an inch, by five silver wires passed and twisted separately; and Dr. Emmet then cut off the mass with scissors, just beyond the row of wire ligatures, and the stump of omentum was then dropped in, and the peritoneal cavity was carefully sponged out. A trocar was then plunged through the Douglas *cul-de-sac*, from above downward, the end passing out at the vulva. A long double silver wire was then passed along the canula of the trocar, and the two were withdrawn. To the end of the wire was attached a bit of India-rubber tubing about ten inches long, and an eighth of an inch in diameter, with oval fenestræ cut alternately along its whole length, at distances of a half-inch or more, according to Chassaignac's plan for drainage-tubes. The wire was drawn to the lower angle of the external wound, holding the upper end of the tube on a level with the brim of the pelvis, while its distal end projected from the vagina for two or three inches. The object of this tube was to secure drainage from the peritoneal cavity. This done, the abdominal incision was brought together by the continuous silver-wire suture; a large roll of cotton wadding was laid on the abdomen, covered with a napkin, and then secured by four or five strips of adhesive plaster. Dr. O'Byrne administered nitrous-oxide gas; the operation lasted fifty-six minutes. The patient recovered consciousness in two minutes after she ceased to breathe the gas.

4 P. M.—Ten minutes after the operation the pulse was 144, and she complained of great pain. Gave ¼ grain sulph. morph. hypodermically.

5 P. M.—Pulse 120; no pain.
6.40 P. M. " 116.
8 P. M. " 112.

There was a complete drainage by the tube of a reddish serum in sufficient quantities to necessitate the constant use of napkins. She had a good night, sleeping under the influence of the morphine.

28th.—Pulse from 112 to 114; temperature, 99. Morphine was given subcutaneously twice a day. She took beef-tea freely, and ate large quantities of ice.

29th, at 7 P. M.—The pulse was 120, but she was cheerful and her expression was good. She complained of some pain, but morphine never failed to relieve it; at midnight her pulse had fallen to 100. Ever since the operation there has been a constant drainage of a dirty, brownish, inodorous serum from the peritoneal cavity.

A further daily report of the case is unnecessary. She took beef-tea for the first three days, became tired of it, and took a milk diet for a day or two, and then she began to eat beefsteak and chicken. She suffered a little from flatulent indigestion, which baffled the ordinary remedies, but yielded at last to a plentiful use of champagne. Her bowels were moved spontaneously on the fourth day and every three days thereafter. She took morphine (hypodermically) twice a day for more than a fortnight, then once a day for a week or more. Her pulse never rose above 120, and then only two or three times, and for but a few hours. It ranged from 110 to 116 for the first five days, and then it fell below 100. She was cheerful and hopeful during the whole time. Her temperature on one occasion rose to 101, but its usual range was from 98½ to 99½. The wire sutures were removed on the tenth day. They ought to have been removed three or four days earlier, for they produced a small anthrax at the upper angle of the wound. At the end of a fortnight she had a bed-sore, which gave a good deal of trouble, and was slow in healing. About this time her bowels became irritable, seemingly the result of imprudence in eating and of the heated term that we had passed through. In the course of the following ten days she had three or four attacks of diarrhœa, which were promptly controlled by morphine and proper diet.

But the most interesting feature of the case was the working of the drainage-tube. Soon after the operation, the sero-sanguinolent fluid began to pass off by the tube, and the napkins were saturated with it, and necessarily changed frequently.

On the second night a large sheet folded and laid under the hips was wet through and through. The tube was moved up and down a little every day by pulling alternately on the wire as it protruded from the lower angle of the external wound, and then on the end of the tube as it projected from the vagina. Now and then a little warm water was thrown into the peritoneal cavity by the vaginal portion of the tube. On the fourth day the discharge became thicker and of a darker color, but was still inodorous.

On the fifth day I found her person soiled with what appeared to be semi-fluid fæces, which I supposed must have passed involuntarily from the bowels, and I scolded the nurse for allowing her to lie there in such a filthy state. The nurse declared that the discharge came from the tube.

On examination, I found it in the tube and in the vagina. But it looked so much like fecal matter that I introduced my finger into the bowel to ascertain if I had unwittingly passed the drainage-tube through a segment of the rectum where the vagina is reflected from it. It was intact, and I was satisfied that the discharge, which had the appearance of dissolved fæces, really came from the peritoneal cavity. This peculiar, dirty, chocolate-colored, pasty stuff continued to come away for three or four days. It had no disagreeable odor whatever. It gradually disappeared, and after a few days there was a small purulent discharge, with a very unpleasant smell. When the chocolate-looking discharge ceased, I removed the Chassaignac tube and introduced a silver catheter three or four times a day, and injected an ounce of carbolic water through it. Soon after the removal of the tube, water could not be injected in larger quantities than an ounce. It seemed to enter a closed sac, and, if we attempted to inject more than an ounce, it produced so much pain that we were obliged to desist. The discharge, small in quantity, became purulent about the tenth day, and the little cavity that gave rise to it was syringed out three times a day for a fortnight longer. When the operation was performed the adhesions were found to be so extensive, and the patient was so aged in appearance as well as in reality, that no one present thought she could possibly recover; and, but for the drainage-tube, I am convinced that she would have died of septicæmia in thirty or forty hours after the operation. She left the hospital on the 1st of August, and is now enjoying good health.

My next drainage-case is altogether unique:

Mrs. D., aged thirty, married four years, no children, at fourteen years of age was accidentally kicked by her brother in the left iliac region, while they were in a romping play. She lost a great deal of blood at the time from the vagina, and was laid up for many months. As she has never been entirely free from pain in the region of the kick, she attributes to this accident all her subsequent troubles. In 1862, '63, and '64, she had what her physician termed congestion and inflammation of the left ovary. In 1865 she had an attack of enteritis. In 1866 she accidentally ran against the corner of a marble table, which struck her in her weak (left iliac) side. This laid her up for many weeks, and her physician thought her very dangerously ill. She never fully recovered from this attack; always complained of pain; and, soon after her marriage in 1868, she discovered some enlargement of the abdomen. I saw her in January, 1869. She was very pale and quite anæmic. Menstruation irregular, rather scanty, and exceedingly painful. Os tincæ small, cervix small, nipple-shaped, and indurated. Uterus small, but of normal depth. In the Douglas *cul-de-sac* there was an oblong cystic tumor, perhaps five or six inches in its largest or vertical diameter, which elevated the uterus and pushed it toward the symphysis pubis. Its attachments were seemingly to the left of the uterus, but my mind was not clear as to their nature and extent.

I prescribed tonics and generous living, and advised her against any immediate surgical interference. However, I assumed the supervision of the case, and when I saw Mrs. D., ten months afterward, I found that the tumor had grown to be about the size of the uterus at the sixth month of gestation.

In 1870 it did not increase much, but her general health broke down completely, and she rarely left her house. During the last winter (1871–'72), she was so much reduced as to be confined mostly to her bed.

In the spring (May, 1872) she appeared to be failing so fast, that she implored me not to procrastinate the long-promised relief. The hot weather of June prostrated her exceedingly, and she feared to wait till the fall for the operation. I was about to leave New York for Newport for the summer, and Mrs. D. insisted on going to Newport for the operation, provided she should there recover strength enough to undergo it. I consented, and in three weeks she improved so much that she drove to the beach, and was able to sit up two or three hours at a time. With her improvement she became impatient for the operation, and it was done at the Perry House, Newport, on August 3, 1872.

My diagnosis was a single cyst of the left ovary. I was assisted by Dr. Perry, of the Woman's Hospital; Dr. Sands, Dr. Engs, and Dr. Birkhead, of Newport; and Dr. Argyle Watson, of New York. Harry Sims gave her nitrous-oxide gas.

The usual incision was made down to the peritoneal cavity. The tumor presented, but it did not have the pearly appearance of an ovarian tumor. It looked more like the uterus than any thing else, and its serous covering seemed loosely attached by cellular tissue to its deeper and denser structure. Blood-vessels were large and plentiful all over the surface. There were no adhesions anteriorly. The trocar was pushed into the sac, and about thirty pounds of very light amber-colored serum was discharged. To the eye it had the appearance of ascitic fluid. The abdominal incision was enlarged to about five inches, to allow the hand to pass into the peritoneal cavity, at the same time that the sac was drawn out. To my dismay, I found the tumor really a cystic tumor of the posterior wall of the womb, and intimately connected with the broad ligament on the left side.

I was for a moment in doubt what to do. If I had stopped with making the trocar-puncture in the sac, I could have stitched it up with silver wire, and left the case as an unfinished exploratory operation. But I had cut a large, irregular hole in the cyst for the purpose of passing my hand inside, to explore its nature and connections. Dr. Perry suggested to me to remove the uterus with the tumor. The operation was practicable, but I thought my patient entirely too feeble and exhausted to bear the shock, and I felt pretty sure that in her condition she could not recover from it. My dilemma was overcome by hitting upon the plan of pulling the sac out as far as we could through the abdominal wound, and of securing it in the lower angle of the incision just as we do the pedicle.

In making forcible traction on the sac, its peritoneal and fibrous covering was lacerated longitudinally along its posterior surface for about four inches. This was whipped over with half a dozen points of silver wire, which was cut off close, and left to be sacculated. I then made a puncture with a trocar through the left side of the posterior *cul-de-sac* of the vagina into the pelvic cavity, and inserted a self-retaining tube made of hard rubber, about the size and shape of a cock's spur, for the purpose of draining the peritoneal cavity.

Expecting to find a single ovarian cyst, I was not prepared with another tube. I then made a puncture through the bottom of the cyst behind the uterus and through the *cul-de-sac* of the vagina, but, having no tube to leave in the track of the puncture, I introduced a bit of gum-elastic bougie No. 8, about five inches long, with little perforations on opposite sides for drainage. Thus it will be seen that I had two drainage-tubes, such as they were, passing through the roof of the vagina, the first lying in the peritoneal cavity, the second passing through the bottom of the cyst and resting in its cavity. These imperfect drainage-tubes being arranged, it was then time to secure the sac between the edges of the abdominal wound. The cyst, when forcibly drawn out and put on the stretch, was fully as large as the wrist. It was crowded down to the lower angle of the wound, while the incision above was closed for two and a half or three inches with silver wire. All that portion of the cyst that was now held external to the abdominal incision, was cut open with scissors in a vertical direction, one-half being pulled over toward the crest of the left ilium, the other half toward the right. Each side was then secured by three silver sutures to the side of the abdominal wound, with which it was in immediate contact. The flaps of the cyst were then cut off, about an inch above the level of their union with the parietes of the abdomen. A large darning-needle was passed transversely through the projecting portion of the cyst, to aid in holding it in a state of tension, and in preventing it from prematurely sinking down into the abdominal cavity. The cut surface of the projecting cyst was covered with pledgets of cotton wet with liquor ferri subsulphatis. A large roll of cotton-batting laid over the abdomen, covered with a napkin, and held down with sticking-plaster, completed the dressing, and she was put to bed, having been under the influence of the nitrous-oxide gas sixty-three minutes. She recovered perfect consciousness in five minutes; she was four minutes getting under the influence of the gas: the operation lasted, then, fifty-four minutes. On her recovery she complained of excessive pain, and, as she objected to the hypodermic use of morphine, I ordered fifty drops of McMunn's elixir of opium. Waiting nearly an hour, and finding her in no way relieved, I gave a half-grain of morphine by the skin, and in a very brief period she was easy; pulse 114.

At 7 P. M. vomiting came on and produced pain in the womb, and bleeding from the cut surfaces of the cyst, which was checked with difficulty. I was obliged to split each flap with scissors into two or three divisions, and tie each of these separately with strong ligatures, and then smear the raw surfaces over with undiluted liquor ferri subsulphatis. I have no idea how much blood she lost—perhaps five or six ounces; certainly too much for one so debilitated as she was. Up to midnight her pulse was from 112 to 119.

August 4th.—Water drawn three times since operation, scanty and high-colored.

3.30 A. M.—Nausea and vomiting; very restless; sweating about the face; pulse 140. All these symptoms supposed to show the need of a dose of morphine; gave ½ grain hypodermically. In ten minutes afterward she was relieved, and the pulse came down to 118, and in thirty minutes it stood at 112. At 3 A. M. the vomiting, which was severe, had so much displaced the drainage-tube on the left, entering the peritoneal cavity, that it was removed; and a few hours afterward, when my patient was tranquil, I attempted to replace it, but it was impossible for me even to designate by the touch the point that had been perforated.

9 A. M.—Some nausea and vomiting; pulse 117.

12 M.—Pulse 120; great retching, but little vomiting, for her stomach is empty, as she has taken nothing since the operation but lumps of ice and a little lime-water. Gave ⅙ grain morphine by the skin.

2.30 P. M.—Pulse 112. She is easy, quite tranquil, and asks for ice-cream.

4 P. M.—Complains of a sense of fulness in the head, left cheek flushed; she attributes this to the morphine, having great prejudice against taking it. Pulse 104.

7 P. M.—Vomited the ice-cream taken a few hours ago.

9.30 P. M.—Vomiting.

11 P. M.—Severe retching and vomiting; pulse 120. Gave ⅙ grain morphine by the skin; thirty minutes afterward pulse 111, and she was sleeping quietly.

It would be too tiresome to make a further daily report of this case. Her recovery is undoubtedly due to close watching, good nursing, and the drainage-tube. On the third day I removed the bit of gum-elastic catheter that had been passed from the vagina through the Douglas *cul-de-sac* into the peritoneal cavity, and on into the bottom of the sac. It had collapsed, and did not answer the purpose of drainage. But at this time there was nothing in the sac itself to drain. On its removal I attempted to introduce a tube of hard rubber, about three inches long, but I could only pass it into the peritoneal cavity. It was impossible to find the opening in the bottom of the sac. However, it answered a good purpose here, for it served to drain a little reddish serum from the peritoneal cavity for five or six days, and I supposed my patient was about to recover without any serious drawback. I threw a little warm water, night and morning, through the tube into the peritoneal cavity, not more than an ounce or two at a time, and repeated these injections until the water would return clear. At the end of a week the reddish serum ceased to make its appearance, and was supplanted by a purulent secretion. At the end of ten days my patient was alarmingly ill. Her pulse rose above 120, temperature to

104½; she vomited constantly when not under the influence of morphine; complained of dizziness and exhaustion, and talked incoherently. She had all the appearances of pyæmic poisoning. With this idea fixed, it was easy to find its source. The cavity of the sac was secreting a sero-purulent matter, which could not escape, as the opening in the bottom of the sac had closed when the gum-elastic catheter was removed. For the purpose of exploration, I passed a small gum-elastic catheter down to the bottom of the sac, through the large opening in the abdominal incision, and, by throwing a stream of warm water through it, two or three teaspoonfuls of turbid sero-purulent matter were washed out, to the great relief of all the symptoms. This was done every three or four hours, and it was surprising to see how the dizziness and other distressing symptoms would be almost instantly removed. Dr. Peaslee reports cases in which all distressing pyæmic symptoms were instantly relieved by washing out the peritoneal cavity. One must see the thing done, to realize the rapidity with which it is followed by relief.

With my best efforts to wash out this long, narrow pouch (five inches deep), I saw that my patient must die unless there was a continuous drain through the bottom of the sac, and through the *cul-de-sac* of the vagina. She was so exhausted that I feared to give her an anæsthetic, even the nitrous-oxide gas, so I resolved to risk the shock of a puncture without the gas. I passed a long, curved trocar, about No. 10, through the external opening of the sac down to its lowest point behind the cervix uteri; then, with a finger in the vagina to feel the trocar, I passed it suddenly through the sac and through the *cul-de-sac* of the vagina and out at the vulva, and by this means a drainage-tube about twelve inches long was drawn up through the sac, one end hanging out over the abdomen, the other projecting from the vagina. This being perforated along the portion that lay in the cavity of the sac and in the vagina, served as a spontaneous and continuous drain. Every three or four hours warm water, often a pint, was injected with a Davidson or Mattison syringe, until it passed through quite clear. It was necessary to move the tube up and down a little at least twice a day, to liberate its little fenestræ from the pressure of the soft parts in contact with it.

This tube was worn for about six weeks, till the canal in which it lay was contracted so closely around it that it was painful to pull it up or down. I then replaced it with one only one-sixteenth of an inch in diameter, which she wore for a month, when it was removed, leaving a silver wire in its stead. The wire was retained only three or four days, and soon after its removal the little fistulous track, so long occupied by the drainage-tube, closed spontaneously. This poor patient has had a hard struggle for life, and deserved to get well. When I thought her entirely out of all danger, she was taken with symptoms of pelvic cellulitis on the left side of the uterus, from which she is just now recovering.

Let it be remembered that this was not a case of ovariotomy. The cyst was uterine.

My third drainage case was an utter failure, resulting in the loss of a most valuable life. But it is not less interesting or less profitable on this account. Mrs. G., aged thirty-six, the mother of five children, had enjoyed good health till about three years ago, when she discovered some enlargement of the abdomen, and complained of a sense of bearing down.

On consulting her family physician, Dr. Owens, of Lynchburg, Virginia, he detected a tumor which he thought was ovarian. The abdomen continued to enlarge till Dr. Owens was obliged to tap her last April, drawing off about thirty pounds of ascitic fluid, leaving hard tumors, now more easily to be felt in connection with the uterus. In May Mrs. G. went to Baltimore and consulted the eminent Prof. Nathan R. Smith, who advised her against any surgical interference. Soon after this she came to New York to see me, and I found the abdomen rapidly filling again with ascitic fluid. There was also a hard tumor about the size of a quart measure in the right iliac region; one end of it appeared to be attached in in the neighborhood of the right side of the uterus, while the other floated in the ascitic fluid and reached up toward the right hypochondrium. The uterus, of normal depth, was pushed forward and above the symphysis pubis by a hard, rounded tumor in the Douglas *cul-de-sac*, which appeared to be continuous with another equally hard tumor in the left iliac fossa. These tumors felt as hard and inelastic as fibroid tumors ever do. I thought they were fibroids, and saw no hope of doing any thing for her relief. Dr. Emmet and Dr. Thomas were called in consultation, and agreed with me that the tumors were probably fibroid, and advised me against any operative procedure. The abdomen continued to grow larger and larger, and soon it became necessary to tap her again, and, in about eight weeks after the tapping by Dr. Owens, I drew off another thirty pounds of ascitic fluid. The removal of this fluid allowed a little closer investigation into the nature of the supposed fibroids; and I discovered a soft or rather elastic point, hardly larger than the end of the thumb, in that portion of the tumor resting in the left iliac fossa, and just above the iliac border of Poupart's ligament. Every other portion of the tumor was hard and inelastic. Although I found but a single point of fluctuation, I determined to puncture the tumor, and ascertain the nature of its contents. Accordingly, I passed a No. 2 trocar of Dieulafoy's *aspirateur* through the abdominal walls into the tumor, to the depth of about four inches, and drew off sixteen ounces of an amber-colored fluid, which I submitted to Dr. Waterman, telling him that it was probably from a fibro-cystic tumor of the uterus. He said the spectroscope would determine the nature of the fluid—that if it were ovarian, the spectroscope would demonstrate the presence of lutein; but, if it were fibro-cystic, the lutein would not be found. On the following day Dr. Waterman informed me that the fluid was undoubtedly from an ovarian cyst, as the lutein line was clearly demonstrated by the spectroscope. Considering that the nature of the principal tumor was now settled, I proposed to make an exploratory operation, which was willingly acceded to by Mrs. G.

It was understood and agreed that the peritoneal cavity should be opened, that the operation should end with this if the tumors proved to be fibroid, but, if they were ovarian and removable without more than the ordinary risk, the operation should be completed. Having full faith in Dr. Waterman's spectroscopic diagnosis, I expected to find the tumor behind and on the left side of the uterus ovarian; but the long, narrow indurated one on the right side of the uterus I thought was a fibroid. The operation was performed at Newport, on

August 6, 1872. I was assisted by Dr. Emmet, Dr. Cleveland, and Dr. Nicoll, of the Woman's Hospital, and by Drs. Watson, Engs, Birckhead, and Harry Sims. Dr. Nicoll gave nitrous-oxide gas. The usual incision was made, and, when the peritonæum was opened, we drew off about twenty pounds of clear ascitic fluid. The oblong tumor on the right side of the uterus was to my surprise a cyst, but so hard that to the touch it was as tense as a well-stretched drum-head. Its contents were evacuated, the cyst was enucleated from its sessile attachments, the pedicle thus formed was secured by silver wire, and the two were cut off close. The tumors on the left side and behind the uterus were all cystic, with very firm attachments, and they were all (four) enucleated. I have seldom seen stronger adhesions or a more difficult operation. The posterior surface of the uterus, the left broad ligament, the sigmoid flexure, and even a portion of the rectum, were more or less denuded. The bleeding was very profuse, and several vessels required torsion or ligatures, and the gentle, almost imperceptible oozing of blood from the lacerated surfaces necessitated the free use of the liquor ferri subsulphatis.

The *cul-de-sac* of the vagina was then punctured with a trocar, and the cock-spur hard-rubber tube was passed from the vagina into the peritoneal cavity for the drainage of what I knew was inevitable after such an operation. The bleeding appeared to be checked, and we rapidly closed up the external incision with the continuous silver-wire suture, leaving an opening the size of the index-finger at the lower angle also for drainage. Just as I was placing a bit of India-rubber tubing doubled on itself in the lower angle of the incision to keep it open, a violent fit of vomiting forced a quantity of blood out by the tubing. We quickly pulled out the sutures, opened the abdomen, and found six or eight ounces of blood in the pelvic cavity, which was sponged out, and Dr. Emmet found a bleeding vessel on the right side of the pelvis in the cellular tissue, near the iliac fossa, which he tied with catgut. Waiting a while, and feeling sure that there was no more danger of bleeding, the external incision was closed as before, leaving an opening at the lower angle, which was occupied by the bit of tubing. She was under the influence of the gas one hour and thirty-four minutes. The following notes of the case were furnished me by Dr. H. D. Nicoll :

When placed in bed at 2 P. M., the pulse was scarcely perceptible, and the extremities were very cold. Brandy by the mouth. Hot bottles to surface and feet. She soon began to vomit, and the breakfast, taken four hours before the operation, was thrown up.

2.45 P. M.—One-sixth grain morphine by the skin; brandy and beef-tea by the rectum; pulse very feeble.

5 P. M.—Vomiting less; patient very restless; one-fourth grain morphine by the skin.

9.30 P. M.—Very decided reaction; skin warm, moist; pulse 135. Has slept for two hours and a half; vomits occasionally. Has taken nothing but some bits of ice; no pain over abdomen; has passed a little urine twice.

August 7th, 3 A. M.—Has dozed occasionally, very restless; pulse 150 and very weak; one-fourth grain morphine under the skin. Vomits about once an hour, throwing off each time an ounce of greenish, transparent liquid; surface beginning to grow cold again.

4 A. M.—Surface colder. Pulse very feeble and frequent. Brandy and beef-tea by rectum.

5.30 A. M.—Pulse 160, scarcely to be felt. Great restlessness. Extremities cold. Vomits every twenty minutes.

6.15 A. M.—One-quarter grain morphia hypodermically.

7 A. M.—Pulse 150. Extremities getting warmer.

7.30 A. M.—Beef-tea and brandy by rectum.

7.45 A. M.—Effect of enema perceptible in pulse, skin, and countenance. General condition seemingly better. Pulse still 150, but stronger.

8.30 A. M.—Some warm water thrown into peritoneal cavity through the drainage-tube, which returned with a reddish tinge.

9 A. M.—Beef-tea and brandy by the rectum. Patient dozes and vomits occasionally.

10 A. M.—Pulse 140, respiration 24. Repeat beef-tea and brandy. One-fourth grain morphia by the skin.

12.30 P. M.—A little bloody serum has passed from the vagina. Threw six or eight ounces of warm water through the tube into the peritoneal cavity, which returned highly sanguinolent. Pulse scarcely perceptible; voice strong; expression good. Temperature 107.

1 P. M.—Beef-tea and brandy by rectum.

2 P. M.—Twenty-four hours since operation. Cold, clammy sweat.

3 P. M.—Washed out peritoneal cavity by vaginal tube; water returned highly colored with bloody serum. Temperature 103½.

4.30 P. M.—Peritoneal cavity again washed out; water highly colored; some blackish granules of iron and blood.

5 P. M.—The tube has not answered the purpose of spontaneous drainage at all. Removed it, and introduced a gum-elastic catheter No. 6, and threw a half-pint of water into the peritoneal cavity, which returned highly colored. Tube replaced to keep puncture in *cul-de-sac* open.

6.15 P. M.—Brandy and beef-tea by enema. Temperature 104, pulse imperceptible.

8 P. M.—Arms cold and clammy to elbows. Removed tube. Peritoneal cavity washed out through a catheter; water returned reddish. Tube replaced.

9.45 P. M.—Temperature 104. Brandy and beef-tea by rectum.

10.30 P. M.—Has been sleeping; waked up complaining of pain in the bowels. No pulse; seems to be unconscious.

Midnight.—For the last four hours there has been no discharge from the vagina, nor has there been any since the operation except when water was thrown into the peritoneal cavity by the tube. The tube was now removed, and there was a sudden gush of reddish serum from the peritoneal cavity, and it continued to flow away in quantities sufficient to wet napkins and to soil the bed-linen. Impossible to say how much reddish serum was discharged in an hour. It might have been a pint, or it may have been only half that quantity. However, it gave no relief whatever to the general symptoms of suffering and prostration, and she continued to sink, and died at 10 A. M., forty-six hours after the operation.

Post mortem at 4 P. M., August 8th, six hours after death, made by Dr. Nichols, of Boston, and Dr. Walker, of the Woman's Hospital, assisted by Dr. White, of New York, and Harry Sims.

Abdomen opened by cutting the parietes from the crest of one ilium up along the margin of the short ribs, then across the epigastrium and down on the opposite side to the other iliac crest. The flap thus made was turned down over the thighs so as to expose the peritoneal surface of the abdominal incision. This was six and a quarter inches long, closed by seventeen sutures of silver wire, and was perfectly united through its whole extent down to the lower angle, which had been intentionally left open for drainage. Peritoneal cavity filled by a reddish, sero-sanguinolent fluid measuring twenty-one and three-quarters ounces; about one-third of this was in the sulcus between the lower margin of the liver and the transverse colon, the balance generally diffused over the peritoneal cavity. The pelvic viscera were blackened by the large quantity of iron used to arrest the oozing of blood; and there was a sort of amalgam of granulated blood and iron in the Douglas *cul-de-sac*, which effectually prevented the sero-sanguinolent fluid above from passing out through the drainage-tube.

The cock-spur drainage-tube was the worst contrivance that I could possibly have devised for the purpose. It had no opening in the upper end, only fenestræ on each side, and these were doubtless closed by the exudation of lymph and by the pressure of the soft parts; for it will be remembered that there was no spontaneous drainage at any time by this tube. On three or four occasions water was injected through it into the peritoneal cavity, and always returned of a reddish color. At midnight, ten hours before death, the tube was removed entirely, and a large quantity of reddish serum came away, probably all that had gravitated into the pelvic cavity.

When the operation was finished, the case was looked upon as almost hopeless by Dr. Emmet and myself. With my ineffectual drainage apparatus it proved to be absolutely so. With the Chassaignac tube running from the external incision down through the *cul-de-sac* of the vagina, there would have been some chance of recovery; but, with the stupid arrangement I had, there was none. Indeed, she died precisely as she would if there had been no puncture in the *cul-de-sac*, and no effort at drainage at all.

ART. II.—*Thermometry in Health and Disease.* By F. C. CURTIS, M. D., Albany, N. Y.

WITHIN a few years, comparatively, various mechanical contrivances have been devised for assisting in the diagnosis and prognosis of diseases, and for obtaining indications for treatment. The thermometer is one of these physical assistants, the clinical use of which is the subject of this paper. Like all the others, it is only an assistant, but is at least that, and I believe its use should be more extensive than it is. Outside of the hospitals I doubt if it is used much, and, while the hospital is the place to study the instrument, because the patients are there in a body and always close at hand, still it is very portable; and, although regularity of observation is a vital point in its use, yet a single observation is often of great value.

The instrument used is a modification of the ordinary mercurial thermometer. A thermo-electric apparatus has been devised which surpasses in delicacy the best mercurial instrument, and by which the temperature of internal parts can be obtained. It consists of two wires, of different metals, soldered together, and having their free ends brought into communication with a thermo-electric multiplier. The wires are passed into the tissues like acupuncture-needles, and the temperature of the point of contact of the two metals is indicated. It has thus been found that the body is cooler toward the surface. But for obvious reasons this apparatus is only adapted to scientific investigation, and the mercurial thermometer is the only instrument of practical usefulness.

Great care should be used in getting an accurate thermometer, especially if the observations are to be preserved for reference. There may be good American instruments made, but I have never seen one. I found in the New York Hospital three, of New York makers, all of which varied nearly a degree. A fault which renders an instrument nearly useless is an unequal variation between different degrees. I believe the best—most accurate as well as portable—is that made by Casella, of London. The Germans make very good instruments, but as a rule they are graduated to the scale of Réaumur or Centigrade. Casella's are carefully made and tested, and are graduated to fifths of a degree. The instrument should be sufficiently delicate to reach its maximum in three minutes, except in a very emaciated subject; some will continue to rise for ten minutes. It is best to have it self-registering. In these the index consists of half an inch of mercury detached from the rest by a minute speck of air. This avoids the necessity of stooping over in close contact with the patient in such diseases as typhoid, which are more or less contagious; the sick-room too is often darkened so that it is not easy to read it *in situ;* it can be trusted to the nurse for one of the daily observations better than tho non-registering instruments.

The axilla is the usual point for taking the temperature, and if another point is chosen it should be noted, as the temperature differs. It should be properly inserted. On the brachial side of the axilla, a little down, it will be noticed that a fold forms as the arm is carried across the body. Lay the bulb in this place, first drawing the clothing well away, and then carry the hand over to the opposite shoulder, the object being to surround the bulb entirely by living tissue. With female patients who may feel a delicacy toward exposure, direct the nurse to lay a towel over the chest before opening the clothing enough to insert the bulb.

Although single observations are often valuable, it is much better to take them continuously. The relation of morning to evening temperature and the range from day to day are very valuable, the main usefulness of the instrument being here. If taken continuously they should be regularly, from 7 to 9 o'clock in the morning, and 5 to 7 o'clock in the evening.

The clinical thermometer indicates pyrexia and measures its amount with accuracy. The value of thus accurately determining the amount of heat in disease was first insisted on by De Haën, of Vienna, about a hundred years ago. Heat of body above the normal has been noted as indicating disease ever since medicine was practised, and is generally recognized by the people as pathological. But the only indication of it has been the hand of the doctor, or the sensations of the patient. Neither of these is reliable; the former is only comparative, varying with the warmth of the hand—the latter cannot be depended on, as is shown in a chill, when the body feels cold to the patient, though really the temperature is elevated. I had been taking the temperature of a patient with erysipelas for several days, the case had gone on to convalescence, when, one evening as I came to take it again, I found the patient in a chill initiatory of relapse, and while in it I found the temperature above what it had been, and 2° above normal. It may be that in these cases the skin is really colder from the nervous contraction of the blood-vessels, but the blood is warmer and begins to be so before the chill comes on.

But, aside from the mere detection of pyrexia, which of course can in most cases be detected without an instrument, there is the accuracy of measuring it which can be gained by it alone. The value of this I can show farther on, in getting the typical range of diseases, in finding the extremely high ranges, as 108° or 110°, which by the hand cannot be distinguished from considerably lower ones, etc. Any thing which makes medicine less an art and more a science, and does away, in any degree, with the vast amount of uncertainty which surrounds our practice, ought to be cherished as it deserves.

Pyrexia is abnormal elevation of temperature. This definition implies the possibility of changes within the bounds of health, and such is the case. The normal temperature of the axilla of adults is 98.5° and the range of healthy fluctuation is quite limited. Climate, exercise, different hours of the day, are some of the agents affecting it. Dr. John Davy, who experimented largely, found variations between temperate and tropical regions. The average temperature is nearly a degree higher in tropical climates. In temperate, the *maximum* temperature is found in the early morning, being on the average 98.7°; falling steadily through the day until midnight, when it is 97.9°. In tropical regions the minimum temperature is in the morning, being 98.05° on an average, from which it rises steadily until night to 99°, a variation of nearly a degree. The variations of temperature of the air do not necessarily affect it, though exposure to extreme heat or cold may do so. Active exercise short of fatigue raises it; beyond this point it lowers it. It falls after a full meal, but rises as digestion advances. It is thus seen that variations of temperature between 98° and 99° do not necessarily indicate disease.

There are other variations, peculiar to early life, to the puerperal state, which is in the border-line between physiology and pathology, and also there is difference of temperature in different parts of the body. Dr. Forster gives, in the *Journal für Kinderkrankheiten*, 1862, the result of an extensive series of observations on the newly-born, the temperature being taken in the axilla. At the moment of birth the temperature is the same as that of the mother; it falls directly until in from fifteen minutes to two hours it reaches 28.97° Réaumur or 97.18° Fahr., the average of all taken; the lowest taken was 95.5°. After this fall, immediately following birth, the temperature gradually rises for thirty-six hours, reaching a height of 99.6° Fahr. After the thirty-sixth hour it falls somewhat, but slowly, for four days, when it reaches about the temperature of the healthy adult. It soon rises again, from the fifth to the eighth day, but this oscillation is slight in amount, reaching an average of 99°. It falls somewhat after this, but the temperature of young children is as a rule higher than that of adults. The daily range too is greater, amounting to a variation, according to Mr. Finlayson, of 2°. According to the same observer, there is invariably an evening fall in temperature, being lowest between 7 and 9 P. M., and highest about 4 A. M., there being during the day but trifling variation. These normal variations should be borne in mind when using the thermometer in children.

In the later months of pregnancy the temperature of the body is somewhat increased, according to Mr. Squier, in the London *Lancet*. It is not raised much by a natural labor, any more than strong exertion of any other kind affects the person in health. Toward the close of labor the vagina shows an elevated temperature, in one case being noted as high as 99.9°. In prolonged labor, where muscular exertion is severe and continued, a temperature of 102° is sometimes reached, which can hardly be called normal. It subsides soon after labor. The slight febrile action coming on two or three days after labor, called milk-fever, has an elevation of temperature variable both as to height and duration. It is more prolonged in primiparæ, and has been noted as high as 102° or even 104.3°, lasting in the latter case for twelve days, though the breasts were not affected. A sore nipple, or slight inflammation of the breast, raises the temperature.

Different parts of the body have different temperatures. There is a difference of 1° between the covered parts of the surface, as the axilla, where the bulb may be entirely surrounded by living tissue, and the cavities which open externally, the mouth, rectum, and vagina. I believe the lungs are cooler than the abdominal organs because of the entrance of outside air, and there is a difference in temperature of the blood in the two sides of the heart. The blood coming to the heart by the superior vena cava is of lower temperature than that of the inferior. A recent number of the London *Lancet* contains the interesting fact that the cranial cavity is of lower temperature than the body generally. There is a difference of $\frac{1}{10}$° Centigrade, or more than one degree Fahr., between the cranial temperature and that of the rectum. Chloroform lowers the temperature of the body generally, and this is more marked in the cranium. The same is true of chloral, and of morphia in large doses, though small doses increase it. But in poisoning by alcohol the cranial temperature rises above that of the rectum. The experiments were done on dogs and other lower animals, but how the temperature of the cranium was taken is not stated. Mr. Blake (*Medical Times and Gazette*) found a difference of from $\frac{1}{2}$° to 1° in the left side of the body over the right, but only during exertion. It was more marked when exercising under a powerful sun.

In this connection I may also speak of *post-mortem* elevation of temperature. De Haën, who first used the thermometer, observed a rise of temperature in the death-agony, which persisted after death. The point being forgotten, was recently reëstablished, and a prize-article written on it by Valentin, at the University of Berne. Experiments were made on various lower animals. It was found to be true in all cases. It is due both to an increased development of heat, and to diminished loss of heat. The vital processes which ordinarily develop heat during life persist after death—that is, after the heart ceases to beat—especially certain nervous influences; and probably *post-mortem* disintegration and decomposition may aid in the development of heat. *Rigor mortis* is thought to have but little influence. It would be interesting to know how the *post-mortem* temperature is after sunstroke, in which disease the body-heat is very high, and after death there is no *rigor mortis*.

The retention of heat after death is favored by the cessation of perspiration, and the consequent lessened evaporation from the skin. Evaporation of perspiration is the safety-valve in life to those exposed to heat. As long as this is active, there is little danger of the high ranges of temperature. In sunstroke it is stopped, and when the body-heat reaches 110°, as reported by Wilson Fox in rheumatism, the perspiration, which is usually excessive in this disease, has stopped.

I have thus, somewhat rapidly, sketched those variations and conditions of thermometry which are outside of that portion of the subject which considers them in the sick patient. We find the healthy body maintaining a temperature varying very little, though surrounded by an atmosphere which changes from day to day, which in the course of the year goes through wide ranges, and in the torrid zone has a constant temperature very far removed from that of the frigid. The source of this animal heat is in the tissues themselves, in which there is constantly going on an endless variety of solutions, combinations, and decompositions, by means of which is effected the metamorphosis of food into tissue, and of tissue into effete matter. But the amount of this heat-producing tissue-change varies continually; to prevent a corresponding increase of heat, we have the cutaneous perspiration, the evaporation of which causes the heat to become latent. So the temperature is continually regulated, and the almost unvarying standard maintained. Variation from the standard in disease is due to change in the one or the other, or of their relation to each other.

What is the advantage afforded by the use of the thermometer in disease?

In the first place, it shows whether or not pyrexia actually exists. This seems to be a small matter, but really it is often a valuable item of knowledge. Often diagnosis is very difficult, especially with children, who may present grave symptoms on slight causes. For instance, not long ago I saw a boy ten years of age who had been taken with convulsions, headache, vomiting, and loss of appetite; the bowels were regular, the pulse irritable, but not accelerated, and he lay quietly in bed, with a peculiar nervous look of the eyes. I found his temperature quite normal, and concluded that his convulsions were due to peripheral irritation, not to central lesion. His convulsions were relieved by a full cathartic, and their cause was found to be eating hard Bologna sausage. There are many cases where the thermometer will give this negative help.

It has another advantage which belongs to it alone; it shows, *before other symptoms can*, the existence of disease and changes in the course of disease. The temperature of the body is more sensitive to diseased action than either the pulse or respiration. In intermittent fever the temperature begins to rise several hours before the exacerbation comes on, and after the disease has been reduced by treatment, and all other symptoms have disappeared, a periodic rise of temperature may be noted, showing that the cure is not yet perfect. In typhoid fever the daily rise and fall of temperature is very regular, and any marked deviation from this is always premonitory of something new in the course of the disease. A sudden reduction is said to show that hæmorrhage of the bowels has taken place, from sloughing of Peyer's patches, and several days may elapse before we get evidence of it from the stools. In pneumonia a sudden fall in temperature about the seventh day shows that convalescence has set in, though other symptoms may be as grave as ever. In all acute diseases a fall in temperature is as a rule a favorable sign.

The converse is in like manner true; rise of temperature, after it has once fallen, points to a relapse. We see this mainly in diseases, as erysipelas, which are liable to relapses. This rise will be found to take place before the premonitory chill, and will be a marked elevation above the regular rise and fall of the steady course of the disease.

There are two or three general rules of temperature in disease which may be spoken of here: First, there is, as a rule, evening exacerbation, the evening temperature being higher than the morning. In some acute diseases, however, the line of thermometric range is steadily upward for a few days, as if carried along by the impulse of the disease. But usually, even in the most acute diseases, there is a fall in the morning from the temperature of the preceding evening; we know that it is common for all sick people to feel better in the morning, and worse as the afternoon wears away.

There is another peculiar fact, familiar to all who use the instrument, though I have not seen it noted in the books. In some acute diseases the temperature falls *below* the normal standard of health as soon as the acute stage is passed. It has been noted below 95° under these circumstances. There are but few other conditions in which a temperature below that of health is noted. Dr. E. R. Hun found it so low as 94.5° in a case of Bright's disease, in which the œdema was excessive, a few hours before convulsions occurred, in which the patient shortly afterward died. The lowest temperatures have been found in shock. This, from all causes, operations, fractures, burns, etc., seems to be attended with more or less depression of temperature. In the "Biennial Retrospect" of the New Sydenham Society, for 1871, Mr. Wagstaffe is recorded as having found, in a case of cut-throat which recovered, a depression to 91.2°, due probably to loss of blood. In a fatal case of fractured spine, a fall of 16.65° was noted. This gives us a hint that nervous influences have a part in maintaining and regulating animal heat.

The thermometer is a help in prognosis. The fact has been alluded to under a previous head that it shows change in the course of disease before other symptoms can. Individual diseases have their peculiarities of thermometric range, and a marked variation from this indicates a grave or favorable termination. In typhoid fever an evening temperature of 103.5° shows a mild case; if it rises to 105°, dropping only to 104° in the morning, it is very grave. In pneumonia, 104° shows a severe attack, especially coming on early in its course. All diseases are not alike in regard to the height to which the thermometer may go within the bounds of safety. Cases of sunstroke have been recovered from in which the temperature of 110° has been reached; and Dr. Wilson Fox reports, in the London *Lancet*, the same height, in a case of rheumatism, which recovered. A few weeks ago I took the temperature for several days in a case of pneumonia, and finally found a morning temperature of 104.2°, pulse 112, and respiration 32. The evening temperature of the same day was 102.8°, which, being so decided a fall from morning to evening, was evidence that the crisis was past, although the pulse was accelerated to 120, and the respiration to 48. The fall here was not sufficient for the most practised hand to detect. In a case of typhoid, looked upon as grave by the attending physician, I was enabled by the thermometer to give a favorable prognosis.

Can a diagnosis be made by the daily fluctuations of the thermometer? It would not be believed at first thought that any one would ever be driven to this means of diagnosis, but I have seen a case of gummy tumor of the brain which in good hands was thought to be typhus fever, and this instrument would settle the question between them with certainty. Some diseases running a regular course have a typical range of temperature, so that one familiar with the subject, turning over the records of vital signs, can distinguish them one from another. In typhoid fever we see a good deal of regularity of the temperature line. Mounting gradually during the first week up above 100°, it holds there, with slight morning fall, till about the twenty-third day, when a considerable fall takes place, and we see the peculiarity of a nearly normal morning temperature and a high evening exacerbation, making long lines in the fever-curve. Typhus, again, is like typhoid, only the morning fall comes one week earlier. In the eruptive fevers generally the evening exacerbation is slight, the fever line, instead of being zigzag, running in a general straight line upward till a certain stage of the disease, and then downward in a likewise straight line. An intermittent of quotidian type shows a fever-line quite similar to that of the third week of typhoid; the tertian differs only in the skipping of one day. These are specimens of the typical thermometric ranges of diseases. It is evident how often diagnosis, that difficult branch of medicine, may be helped out by the thermometer.

Thermometry gives indications for treatment. An exceedingly high temperature is of itself dangerous to life from its effect on the nerve-centres, and should be treated by active antiphlogistics, regardless of the primary disease. This is illustrated by the cases reported by Wilson Fox, of rheumatism, in which the temperature rose to 110°, and which recovered under the use of cold douches and baths. A very forcible illustration of the value of the thermometer, as a help in treatment, is seen in pericarditis occurring in the course of rheumatism. Every case of this disease should be watched carefully with the thermometer, for by it we get premonitions of the occurrence of pericarditis, and are enabled to get the start of it. We are told, and very properly, to listen every day to the heart-sounds, but the fact is evident that, if you wait for cardiac murmurs, you are waiting simply till the serous surfaces are covered with exudation to produce them. I have seen one case in which by most careful examination no murmurs were heard, but the autopsy showed four ounces of bloody serum in the pericardium, and the surfaces covered with a fibrinous exudate presenting the "bread-and-butter" appearance. The serum separated the membranes and prevented friction-sounds. The pulse is sensitive to acute disease of the heart, but it is liable to extraneous disturbance. So, too, with the respiration. And, more than all, thermometric change occurs before any thing else, and so is premonitory, in this and other diseases.

Febrile action is inconsistent with health, and an instrument which indicates and measures it, translates what is going on in the system. The fever is in many cases a safer guide than any thing else. The pulse is disturbed by nervous excitement; exertion increases the respiration; the countenance gives valuable indications in sick children especially, but often it tells nothing; the history cannot always be gained clearly. But the temperature can always be taken, and has one advantage over most other means of diagnosis: it is uninfluenced by extraneous circumstances, and always tells the truth. Its language is to be learned by study and observation.

To sum up in a sentence the valuable points of the thermometer: it shows the existence of febrile exacerbation; it is premonitory of change in the course of a disease; it helps in diagnosis, prognosis, and treatment; and its indications are uninfluenced by any outside circumstance whatsoever.

Clinical Records from Private and Hospital Practice.

I.—*A Case of Tinnitus Aurium of Four Years' Standing; the Result of Congested Tympanic Cavity from Tubal Catarrh; greatly relieved by Injections of a very Strong Solution of Nitrate of Silver into the Eustachian Fossa by Means of the Faucial Eustachian Catheter.* By O. D. Pomeroy, M. D.

Hannah McGloin, aged twenty-six, a feather-maker, applied at the Manhattan Hospital, on December 15, 1871, for the relief of a most distressing tinnitus aurium, which had troubled her for four years, but not until nine months since did the present aggravation of symptoms exist. So annoying was the symptom that her sleep at night was often prevented; and, indeed, at first I prescribed large doses of opium, in order to afford temporary relief. It is worthy of remark to mention here that the tinnitus never has affected her while sitting upright. The noise was confined to the right ear. The left was perfectly normal. The H. D. of the right was ½ (4/8), increased by inflation to ⅞. The membrane was nearly or quite normal. The throat showed the signs of naso-pharyngeal catarrh; the Eustachian tubes were both carefully examined by the rhinoscope. The left tube showed apparently more signs of disease than the right. Both were often found filled with mucus, and the collar-like post. boundary to the tubes seemed exaggerated; perhaps there was a slight œdematous appearance in the fossa of the tubes. The levator-palati muscle was occasionally seen to contract vigorously, moving noticeably the mouths of the tube.

The treatment at first was leeches to external canal, which produced some light-headedness, without relieving the tinnitus in the least. The bromide of potassium in large doses (10 to 40 grains) was exhibited, somewhat relieving her headaches and quieting her nerves, and possibly diminishing her tinnitus a little. Inflation by Politzer's plan and by catheter was done from the first. After three weeks of his treatment I commenced the injection of 40- to 80-grain solution by means of the faucial Eustachian catheter, ½ to 2 drops being thrown into the fossa of the right tube every second day. This had the effect to render the tube pervious (i. e., preventing the necessity of inflation to improve H. D.), but with very slight influence upon the tinnitus. On January 19, 1872, I commenced the injection of a 480-grain solution of nitrate of silver; at first a fractional part of a drop, slowly increased to one and a half drop every second day by means of the aucial catheter, which at the same time would inflate the tympanic cavity. Under this treatment the tinnitus began to improve within a week.

This treatment has been persisted in until the present time (March), sometimes omitting the regular three-times-a-week visitations. At times, the tinnitus has been so moderate as to give very little annoyance; again, the noise is very troublesome. If the nitrate of silver is omitted for a few days, the symptoms return, but not as severely as before.

I can come no nearer an explanation of the cause of the tinnitus than to infer a hyperæmic state of the tympanic cavity and possibly of the nerves, which may readily result as a consequence of the tubal catarrh, as has been frequently pointed out by Von Tröltsch and others.

The slightly-sunken drum-membrane was evidently not a cause of tinnitus, as its restoration had no effect in modifying it.

A few times I used the ordinary catheter in the nostrils, and exactly fitted into the tube, using the same solution as just mentioned, which passed well up the Eustachian tube, and perhaps some of it into the cavity, but which rendered the tinnitus worse, and caused some pain, indicating to my mind that it is not advisable to throw remedial agents into the tympanic cavity unless some special indications exist for so doing.

August, 1872.—The case is still under occasional observation, and has a very moderate degree of tinnitus. The membrane has within three months been punctured three or four times: at first it seemed to do harm, but subsequently a benefit certainly has resulted, so that at the last visit the tinnitus has annoyed so little that she has discontinued her visits for some weeks.

This case may seem complex; the nitrate-of-silver treatment after a while seemed to have lost its effect, although the evidence of its remedial action cannot be denied. The punctures of the membrane were of undoubted assistance, but on the whole I believe the nitrate of silver to have been the main reliance. At some future time I may report on the results of paracentesis of the drum-membrane, together with the use of Politzer's eyelet to render the puncture patulous.

II.—*Suppurative Inflammation of the Tympanic Cavity, with Subjective Symptoms of Mastoid-Cell Inflammation, treated successfully by Incision, and the Instillation of a very Strong Solution of Nitrate of Silver upon the Drum-Membrane.* By O. D. Pomeroy, M. D.

Mr. J. B., aged thirty-nine, on February 10, 1871, applied at the Manhattan Eye and Ear Hospital, for a very severe pain in his left ear, which had not permitted sleep for the last ten days, although he had resorted to different kinds of treatment for its relief. The external auditory meatus was so narrowed by inflammation that inspection of the drum-membrane, even by a narrow and flattened speculum, was impossible. There was pain, on movement of the jaws, on any attempt to open the mouth; the mastoid region was somewhat reddened, swollen, and œdematous, with decided tenderness on pressure.

Tinnitus aurium was present. The throat was slightly inflamed, although the Eustachian tube was impervious, probably from swelling of its tympanic orifice; the H. D. (hearing-distance) was nearly obliterated.

The tissue over the mastoid was freely incised to the bone, dividing the periosteum to the length of 1¼ inch, which resulted in complete relief to the pain.

The incision was kept open two or three weeks by a tent. A poultice was applied during the next week; if the wound closed at all, it was opened by passing a probe through it. For the next four months the ear discharged muco-pus constantly in spite of the treatment. The drum-membrane being perforated, granulations soon made their appearance, which were destroyed by torsion, touching with strongest nitric acid, and saturated nitrate of silver. The Eustachian tube was kept open by Politzer's operation, and the application of nitrate of silver to its faucial extremity.

In subduing the inflammation of the tympanic cavity, nitrate of silver, 40 to 80 grains to the ounce of water, was dropped into the external canal, a large portion of the time above mentioned, which often passed freely through the tympanic cavity, and Eustachian tube, into the throat. Of the means used at this time the nitrate was most serviceable, slightly diminishing the discharge, and increasing the hearing power.

Alum was used in a similar manner, in saturated solutions and in powder. In either case coagulations of the discharge (alum-curds) resulted, which acted as an irritating foreign body, doing harm. Especially was this the case when *powdered* alum was used, the concretions resulting needing to be removed by forceps; a syringing did not always succeed in dislodging them. This may be stated to be the main contraindication to the use of alum in similar cases.

Finding the previous means insufficient to relieve the disease, on the 26th of July, I dropped into the external auditory canal three or four drops of a solution of nitrate of silver the strength of 480 grains to the ounce of water. It caused a slight feeling of warmth and fulness in the ear, but not real pain, the symptoms soon subsiding.

The discharge was entirely arrested by this one application, although the patient has been observed occasionally for months since. The tinnitus aurium from this time has gradually disappeared. The H. D. was normal (finger-nails snapped) 8; after Politzer 12.

The patient applied to me late in the fall of the same year, for some diminution of the hearing-power, which I found to have resulted from a faucial obstruction of the Eustachian tube of that side; no more tinnitus. The perforation of the membrane had gradually healed after the strong instillation. There may be some doubts of the propriety of publishing a result of this kind, and I am ready to assert that I should have had less confidence in the alleged result of treatment if it had been observed by another, but I do not know how the above conclusions can be resisted. The observation was made also by several of the medical officers of the hospital, who unreservedly concurred in the correctness of the statements here made. Nitrate of silver in strong solutions has a wonderful antiphlogistic power, as may be proved in a variety of experiments; if painted on a somewhat superficial paronychia, it frequently discusses it without the formation of pus, and with a rapid diminution of the pain.

In acute inflammations of the drum-cavity, the pain is often promptly relieved by instilling a few drops of the nitrate of silver upon the membrane, and the disease brought to an abrupt termination. As further illustrating the antiphlogistic power of the local application of nitrate of silver, it is worthy of remark that lately, in some of the journals, it has been recommended to be painted upon the scrotum in orchitis.

III.—*Chronic Otitis Media, with Mastoid Disease, complicated by Acute Meningitis; Recovery.* By O. D. Pomeroy, M. D.

Ellen Lamb, aged eleven, has had a discharge from her right ear for the last four or five years. On January 4, 1872, was attacked by a violent pain in the ear and same side of the head, which lasted for the next three days.

The treatment was leeches, warm applications, and opiates internally. The internal exhibition of McMunn's elixir of opium gave most relief. On the seventh day the pain in the head was much worse, although the ear felt better. A hot foot-bath, cold to head, leech to external canal, and mustard to back of neck, relieved, so that she had a good night's rest on the eighth day.

On the ninth day, the head was very hot, and the patient was delirious, starting up from sleep, and talking wildly.

She had three mild spasms on this day. In the evening I found the head cool, face moist, rest of body dry and hot; patient languid, with sluggish pupils, hardly conscious; pulse 96. Ordered two leeches to external meatus, a blister postauric, and castor-oil. Next day conscious a part of the time, but very feverish; otherwise better. On the night of the 9th, pain in head worse, fever high, but no delirium.

Next morning, no pain, but greatly prostrated. Ordered beef-tea and Borden's milk.

I did not see the patient again until the seventeenth day, when I found a large-sized abscess behind the auricle, which was immediately opened, discharging a large quantity of fetid pus. Patient much emaciated and exhausted; confined to bed for several days previous to this date; pupils normal. Ear discharges profusely; ordered syringing with warm water. Next day there was great relief; the mother says, " The fever has left the head ; " intelligence somewhat dull, but more desire to eat; slept well that night, and no more pain except in the head. On the twenty-third day saw patient again. Has had great pain in back of neck for the last four or five days; vomiting for last two or three days whenever she takes any thing to drink; great thirst, head hot, but body cool; ear discharging less; intelligence good. The mustard relieved the pain in the neck for one or two days. Subnitrate of bismuth after a few days relieved the vomiting. Pain in back of neck returned; says " neck is broken."

Twenty-sixth day; vomiting, and pain in back of neck and forehead. The blisters post-auric seem to do no good; is delirious now a part of the time; great thirst; pulse 120, and small; lies in a stupid way, but is easily aroused; pupils normal, bowels constipated, for which castor-oil was ordered. Pain still continuing in back of neck, and blister is ordered to the spot. Beef-tea given.

February 17*th*.—Has had no more pain, or head-symptoms, for two weeks; discharge from ear still profuse and purulent; gaining flesh. On 28th, had a poor night, with vomiting, fever, pain in head and back of neck.

29*th*.—Vomited four times, but eats well with considerable appetite; pain diminished, had a good night, excepting a chill, followed by fever.

30*th*.—Much better, no vomiting, pain in neck and head gone, appetite good; patient quite lively, with a clean tongue.

Subsequent to this date, granulations have been removed from the external auditory canal, which have been reproduced a second time, and are now (May, 1872) entirely removed, leaving a membrane almost entire, except a small perforation in the anterior superior portion, which is constantly growing smaller. The polypi were from the cavity and passed through this aperture. Discharge very slight. H. D. watch, 6″; voice, several feet. The intelligence of the child seems not to have suffered by the attack. The membrane will probably heal entirely, so as to put a stop to the recurrent attacks of suppurative otitis media.

The head-symptoms were conspicuous enough to excite alarm. There may be a doubt as to whether they were genuine, although I have seen patients die with very similar symptoms, even without the pupils being affected. Nothing is more certain than this—that fatal meningitis, accompanied by ear-trouble, may result, with very few true brain-symptoms. The pain in the back of neck may suggest a cerebrospinal inflammation, and indeed many cases are reported in New York of this disease, with scarcely more signs than were present in this case. The disease resembled inflammation of the membranous labyrinth, as described by Voltolini, of Breslau, but the preservation of so much good hearing excluded that diagnosis. Beyond a doubt, the symptoms just recorded oftener result fatally than is generally supposed.

IV.—*Otitis Media; Suppuration, with a Large Perforation, and Granulations, completely healed by New Membrane Formation, and presenting the same Improvement to the Hearing by Means of the Artificial Drum as before the Closing of the Perforation.* By O. D. Pomeroy, M. D.

Nicholas Ryan, aged twenty-five years, applied to the clinique at the Manhattan for a discharge from both ears, of seven or eight years' duration, developed from an acute purulent inflammation of the cavities at that time. He had occasionally suffered pain in the ears during this time, and the discharge had not been quite constant. The H. D. of R. was 3″, and after introduction of Toynber's artificial membrane it immediately rose to 24″. For the next three months the patient was treated pretty constantly three times a week with, first, removal of the most salient granulations by forceps, then cauterizing by nitric acid and nitrate of silver. The nitric acid was used three or four times, the nitrate of silver about seven or eight times, by touching granular surfaces with a 408-grain solution, and a large number of times the nitrate was used by instilling a few drops of the 40-grain solution into the canal so as to inundate the drum-membrane, which after a minute or two would be washed out by syringing with warm water. Tannin in powder was used, with an unsatisfactory result. Alum in saturated solution proved a somewhat useful but harmless astringent. The granulations returned two or three times, which is an experience I have often verified. At one of these periods a granulation had grown to the size of two lines in diameter in one week.

I have seen these polypi return after having been removed six or seven different times. The perforation from which the granulations sprang occupied about one-third the expanse of the membrane, and was kidney-shaped; the concave portion was opposite the extremity of the manubrium mallei. The perforation was perfectly healed in three months by a new formation. It was well polished, showing light points, and resembling the old membrane, except it was somewhat thinner, allowing itself to be considerably moved outward by Leigh's otoscope during astmospheric traction. *The H. D. was now found precisely as at first, and after applying the artificial membrane the improvement was the same as before the healing of the perforation.*

I can only explain the phenomenon by inferring that the insufficient coaptation of the ossicula, before the perforation, was unaffected by the formation of the new membrane, and that the same condition presented itself to the artificial drum-membrane as before the repair. On January 29th it was recorded: "No discharge for six months, except once or twice for only a few days at a time." The left membrane is not yet healed, although the perforation has diminished in size, and the granulations are nearly removed. About eight months after the healing of the perforation of the right, a somewhat striking change had taken place in the drum-membrane. The quite exact demarkation of the new formation, so as to state precisely its location, has disappeared, leaving a decidedly cicatricial appearance, indicated by the wandering, whitish ridges, running in several directions.

The precise spot indicating the perforation has almost disappeared, except in the upper portion, where it is indistinctly seen.

These degenerative changes, it seems to the writer, have taken place somewhat rapidly. The fact, that the artificial membrane has improved the hearing after the healing of the perforation, is not new, but cases which illustrate it are so infrequent that the present one may well enough be put on record.

Proceedings of Societies.

NEW YORK ACADEMY OF MEDICINE.

At a meeting, held November 9th, the subjoined paper was read by Benjamin Howard, M. D., Professor of Surgery in the University of Vermont, etc., etc.:

THE PREVENTABLE DEATHS FROM DROWNING WITHIN THE METROPOLITAN DISTRICT OF NEW YORK.

The following paper is intended to be a brief inquiry concerning the mortality from drowning and suffocation within our own city limits, and the means which are employed to diminish it. As introductory to the immediate facts to be presented, it may be well to observe that the possible continuance of individual life, after all its ordinary signs have ceased, is a fact which has become familiar by repeated confirmation. It is equally well known, on the other hand, that, after individual or somatic death, life may linger in some of the parts of the body, as demonstrated by the continuance in them of secretion and growth. In the rattlesnake the secretion of its poison has been observed not only after the death but also after the decapitation of the reptile. We might quote from very high authorities instances in which animals have returned to life after a continuous submersion of two hours; others, in which the same success has been attained after several hours not in the water only, but also under the ice; and still others in which, after exhumation from a recent grave, human beings have been restored to life and health. Whatever undue importance may have been given to some of these curiosities of medical literature, they have certainly helped to quicken such investigations as have led us to the more definite knowledge we now possess. Our practical experience in resuscitation is confined almost exclusively to cases of apparent death from drowning, in which we know the susceptibility to resuscitation is peculiar. The reason for this peculiar susceptibility may be found chiefly in the absence of those pathological changes common in death from ordinary disease; death in drowning being caused simply by the exclusion of air by the least possible violence until the vital functions have ceased. The possibility of restoration is still further explained by observing the order in which the process of dying in such a case proceeds. Beginning with a diminished excitability of the fibrillæ of the pneumogastric distributed over the air-cells, the cerebrum, medulla oblongata, and the sympathetic, in their order lose their susceptibility; the respiratory muscles, therefore, soon forget to contract, and the lungs become engorged with blood.

As the toxæmia from accumulating carbon becomes more and more intense, organic life slowly retreats from the surface toward the centre, until the final enervation of the heart is complete and it becomes motionless.

It is thus seen that death is gradually effected by—

1. Cessation of respiration.
2. Superficial asphyxia, or pulselessness.
3. Cardiac asphyxia.

Between each of these stages an interval may occur of shorter or longer duration. During such interval resuscitation may be possible.

Neither from the sufferers themselves nor from the by-standers can accurate reports of the duration of submersion or of unconsciousness in actual cases of drowning ever be hoped for.

Thorough experimentation has been made, of course, upon animals alone. It is impossible, therefore, to fix a limit to the period either of continuous submersion, of unconsciousness, or of apparent death, which will allow or prevent subsequent resuscitation. It is not within the intended scope of this paper to present the physiological aspects of the subject, and we will therefore dismiss this point by giving an opinion based upon the report of the Committee of the Royal Medico-Chirurgical Society of London, the reports of the Royal Humane Society, and the personal experience of the author.

If there be positive evidence that the patient was free from syncope, and has been so submerged as to have inspired no air whatever for more than five minutes, the case is probably hopeless.

If, in any case, proper treatment has been continued more than an hour, and no sign of life is yet discoverable, the attendant may cease his efforts at discretion.

The successful cases which have come within the personal knowledge of the author, or have been treated by him, have averaged ten to twenty minutes in the water, and about twenty minutes of subsequent unconsciousness.

The general nature and success of the measures which have been employed in other countries, for the saving of the drowned, will help us to a proper estimate of our own situation and duty in the city of New York.

In Holland, as early as 1768, an ordinance was passed directing rules for the treatment of the drowned to be read in all the churches. In 1774 the Royal Humane Society of London was established, and its example was quickly emulated in many of the principal cities of Europe.

The treatment then was crude; indeed, within the present generation, in addition to injections of tobacco-smoke and warm baths, it included suspension by the heels, or placing the patient face downward across the back of a horse urged into a trot. The only treatment at this moment known to the masses of our country consists in rolling the patient miscellaneously upon the ground, or face downward upon a barrel.

Since the time of the good and great Marshall Hall, and the establishment of the principle that the one remedy is artificial respiration, an entire change in the treatment has universally prevailed.

To such simplicity is the treatment now reduced, that, with a little instruction, it may be applied without delay, by anybody, anywhere, with almost as good results as the case would admit of, under the most favorable circumstances.

Notwithstanding the former questionable character of the treatment, the results appear to have been such as we, by our better methods, might be very glad to emulate to-day in our own city.

Within six years from its formation, the Humane Society of Amsterdam reported the recovery of two hundred persons. Mr. Alexander Johnson, in a report to the Humane Society of London, states that, from 1774 to 1787, a period of thirteen years, eight hundred and ninety-seven instances of resuscitation occurred in London alone. According to the report of Monsieur Pia, the superintendent of the Establishment for the Recovery of the Drowned in Paris, during the sixteen years subsequent to 1772, eight hundred and thirteen persons apparently dead from submersion were completely restored; one hundred and twenty cases only having been treated unsuccessfully.

To give an idea of the good now being accomplished by Humane Societies, we would quote from the only Annual Report, now in our possession, of the Royal Humane Society of London, published in 1862, from which we find that, during that current year, of one hundred and seventy-three persons treated, one hundred and fifty were fully restored, notwithstanding that twenty-seven of the number had intended suicide; that, during the year just past, the Society reports three hundred cases successfully treated, and sixteen cases of restoration from attempted suicide.

Wherever Humane Societies have existed, it has been shown that, of all the lives known to have been imperilled by drowning, the larger number have been saved. By such associations, science and humanity, in this highest type of human effort, have reaped a reward which, for completeness and magnitude, has scarcely been equalled in any other department of human philanthropy.

The Metropolitan District of New York, according to the official report of the Board of Health for 1868, from which the following facts are quoted, includes a water-front of about 167 miles, a great part of which is swarmed with persons engaged in active pursuits.

The number of persons who crossed the New York ferries during the year 1867 amounted to an aggregate of about eighty millions. These were transported between forty-five ferry-houses, stations, and docks, upon eighty-three steamboats; the relative number crossing at the respective ferries being as follows:

NAME OF FERRY.	No. of Boats employed.	No. of Docks and Ferry-houses.	No. of Passengers transported in 1866.
Union Ferry to Brooklyn	17	10	40,000,000
Peck Slip and Grand Street to Brooklyn	13	4	4,100,000
Jackson Street to Brooklyn	1	2	1,800,000
Houston " "	3	2	1,920,000
Tenth Street and Greenpoint	4	2	1,500,000
Thirty-fourth Street and Hunter's Point	5	2	2,300,000
Ninety-second Street and Hell Gate	2	2	125,000
West Forty-second Street and Weehawken	2	1	360,000
Spring and Christopher Streets to Fort Lee	2	2	780,000
Christopher and Barclay Streets to Hoboken	10	2	5,300,000
Pavonia Ferry, foot of Chambers Street	4	1	3,390,000
Desbrosses and Cortlandt Streets to Jersey City	8	2	13,000,000
Dey Street to North Shore, Staten Island	3	4	1,050,000
Liberty Street to Central New Jersey Railroad	5	1	2,100,500
Whitehall Street to Staten Island	3	4	2,200,000
East River to Bay Ridge	1	3	....
Total	83	44	79,925,000

Notwithstanding the usual precautions for the prevention of accidents, the official report of the Board of Health for the same year states: "The number of deaths in New York alone frequently averages one per day by drowning;" that, "during the years 1863 to 1867 there were no less than one thousand and fifty-nine deaths from this cause in New York, exclusive of suicides."

The following table, compiled from the police records, under the complete and able direction of Dr. Elisha Harris, to whom so much is due in this connection, exhibits the number of cases of drowning for the six months ending October 31, 1867, with results:

MONTH.	Rescued alive.	Bodies recovered.	Bodies not recovered.	Bathers included in one of the three forego'g classes.	Total.
May	13	11	4		28
June	5	21	5	4	31
July	15	21	6	4	42
August	17	25	7	6	49
September	14	12	1	1	27
October	18	10	1		29
Total for 6 months	82	100	24	15	206

This, as will be seen, gives an aggregate of 206 cases of drowning within six months.

A table of statistics, more comprehensive than any which has been heretofore published upon the subject, has been kindly prepared for the author by his friend Dr. Charles P. Russell, Registrar of the Metropolitan Board of Health, and includes all the cases of death from apnœa reported to the Board during the past seventeen years:

YEAR.	Asphyxia or Suffocation.	Accidental Drowning, or found in Water.	Suicide by Drowning.	Suicide by Hanging.	Execution by Hanging.
1855	27	186	4	14	..
1856	6	141	3	13	..
1857	34	164	5	10	1
1858	57	161	9	11	1
1859	110	167		10	..
1860	113	185	3	9	2
1861	139	204	1	6	..
1862	112	182	3	10	2
1863	121	266	2	3	..
1864	107	232	1	8	..
1865	115	175	2	4	..
1866	104	226	5	10	2
1867	92	166	3	12	2
1868	95	171	6	32	..
1869	53	172	8	20	..
1870	63	181	11	27	2
1871	45	187	6	18	1
Total	1,393	3,166	72	217	13

From this table we see that, during the past seventeen years, from 1855 to 1871 inclusive, the total number of deaths reported in the city of New York from apnœa, exclusive of executions, numbers 4,848; that, allowing the same ratio for three preceding years, we have an aggregate loss during the past twenty years of 5,700 souls, or more than 285 annually.

What proportion of these deaths were preventable?

This is a question in which the New York Academy is entitled to a special interest, because it is the one Society which, by its constitution, is responsible "for the promotion of the public health" of this community.

We will approach the question concerning this rate of mortality by asking first, What has been done to prevent it? and, if the facts to be presented imply blame, we ourselves would be the last to claim exemption.

During this twenty years past, the first step toward an organized effort for the rescue and resuscitation of the drowned was taken in the summer of 1868, when the Sanitary Committee of the Board of Health had referred to it the following resolution:

> *Resolved*, That, in view of the large and increasing number of deaths by drowning in the Metropolitan District, as appears from the weekly report of the Registrar of Vital Statistics, the Sanitary Committee be requested to report whether any and what measures can be adopted by this Board to diminish the number of such casualties in the Metropolitan District.

The committee reported that they had carefully examined into the subject referred to, and that "it appears very desirable not to delay the needed action thereon."

After consultation and experiment, a form of apparatus was decided upon, consisting of ladder, pole, and rope. These, in sets or parts of sets, were then distributed and put under the charge of the police at the following points as designated:

List of Places where Apparatus was placed in 1868.

Pier 1, North River, full set.
Pier 10, North River, pole and ladder.
New Jersey Ferry-House, pole and ladder.
Pier 43, North River, pole and ladder.
Pier 47, North River, full set.
Hoboken Ferry, full set.
Foot of Gansevoort Street, full set.
Foot of Cortlandt Street, full set.
Pier 29, North River, rope.
Pavonia Ferry, full set.

Foot of Harrison Street, full set.
Desbrosses Street Ferry, full set.
Grand Street Ferry, full set, and ladder and pole.
Gouverneur Slip, rope.
Clinton Street, rope.
Rutgers Street, full set.
Catherine Street, full set.
Roosevelt Street, full set.
Pier 24, East River, full set.
Fulton Ferry, full set.
Wall Street Ferry, full set.
Pier 12, East River, pole and rope.
Coenties Slip, ladder and pole, and full set.
South Ferry, full set.
Yonkers, full set.
Astoria, full set.
Hunter's Point, full set.
Greenpoint, full set.
Houston Street, full set.

After two or three months, the Board of Health was compelled to suspend all active measures in this direction in consequence of other more imperative public exigencies, and it has been unable since to resume them. The nature of the measures adopted, and the total means employed during that summer, are given in the latter part of a special report made at the end of that year to the superintendent, and published in the Annual Report of the Board for 1868. It is as follows:

> The means hitherto employed to prevent death from drowning, and but recently inaugurated by your honorable Board, have consisted of one temporary course of instruction on "The Rescue and Resuscitation of Drowned Persons," as above mentioned. These lectures have been attended by 819 policemen, 92 of whom are sergeants. Next, the supply of the Board's rescue apparatus, consisting of ladder, pole, and heaving-line, to twenty-four points at the lower part of the city, and the distribution of printed circulars entitled "Rescue of Drowning Persons." I beg respectfully to submit that the great annual mortality from drowning might suggest the economy of adopting a system of practical instruction for the rescue and resuscitation of drowning persons, which should be constantly maintained and become permanently established. I have the honor to be, very respectfully,
>
> Your obedient servant,
> BENJAMIN HOWARD, M. D.,
> *Assistant Sanitary Inspector.*

From a foot-note at the bottom of the page whence this quotation is taken, we quote: "At the date of printing this report, January 18, 1869, twenty-five persons, during the preceding five months, have been rescued from drowning by the agencies of the Board."

This, as far as we can ascertain, comprises all that has been attempted during the past twenty years to diminish the current mortality from suffocation and drowning within our Metropolitan District.

In order to secure the latest information, a note of inquiry was recently addressed by the author to the chief executive of the Board, by whom the following note was kindly sent in reply:

> BUREAU OF SANITARY INSPECTION, HEALTH DEPARTMENT,
> NEW YORK, *September* 14, 1872.
>
> MY DEAR DOCTOR: I have been out of town, otherwise your note would have received earlier attention. In reply to your question, I would say that no active measures are now being taken beyond what was done by the old Metropolitan Board in furnishing an apparatus, with which you are well acquainted, to the several ferries and other convenient places along the water-side, to be used in rescuing persons from the water. So far as I have been able to learn from recent inspection, these are in order and occasionally resorted to, though many prefer a simple boat-hook before any other apparatus.
>
> Truly, etc., E. H. JANES.

In order to exclude, as far as possible, all sources of error, the author has just made a personal inspection of all the points within the city where rescue-apparatus is reported to have been placed, and put in charge of the police there on duty.

With regard to the police surveillance: Two police-officers only were found who could state where any apparatus on their posts had been placed, or where such of it as remained could be found.

After careful, and in some cases very tedious search, all the apparatus and parts of apparatus of the Board of Health, which could be found, consisted of:

2 framed "Rules for Resuscitation,"
7 ladders,
9 poles,
7 ropes.

Thus the entire arrangements now maintained by the Me-

tropolitan Board of Health within this city comprises less than
9 sets of apparatus,
and these are in great part broken or unfit for use. In no case was there found one set in good order and complete.

In no case was there found any conspicuous notice to attract public attention to the apparatus or to designate its use.

Some of the apparatus which was found had been of great service. The author saw two or three parts or sets which had been several times used successfully in the saving of life. He also saw apparatus which had failed on account of its broken condition, loss of life occurring in consequence.

If the want of apparatus is great, this inspection tour showed that the general need for information upon resuscitation is still greater.

Upon very extensive investigation, not one person, from the highest official downward, could be found who, in a drowning casualty, would have attempted any thing beyond rolling the unfortunate patient upon the ground, or across a barrel.

Without exception, however, information upon the subject seemed to be considered by all classes of great personal importance, and is earnestly desired.

The officer in command of the Harbor Police boat said he was meeting with drowning accidents every few days, and thought, if he had possessed the necessary knowledge, he could have restored a good many such persons to life.

This statement was repeated substantially by several other persons about the docks, the statements being accompanied with interesting recitals in evidence.

The rate of mortality from drowning, so high now, as the population increases, must ever tend to be higher.

The causes of this high rate of preventable death from suffocation and drowning which suggest themselves, are chiefly as follows:

1. The public have no recognized source of information concerning rescue and resuscitation.

2. Rescue-apparatus is insufficiently provided and maintained.

3. The members of the Police and Fire Departments, to whom the opportunities of rescue from suffocation and drown-

ing most frequently occur, receive no special instruction to fit them for such emergencies.

4. Life-preservers and fire-escapes are merely articles of merchandise, subject to no authorized surveillance.

5. There is no established means for the proper recognition and reward of heroic effort in the saving of human life, but of animal life only.

We have thus pointed out the peculiar susceptibility to resuscitation which may exist after apparent death from suffocation or drowning. We have shown that the mortality in New York from these causes averages two hundred and eighty-five annually, giving an aggregate for the past twenty years of five thousand seven hundred deaths.

That, in cities where there is an organized system for this purpose, the majority of persons thus imperilled are rescued; that, of those rescued in 1862 by the agencies of the Royal Humane Society of London, ninety-one per cent. were restored to life and health; and that, during the past year, the same Society reports three hundred cases successfully treated, sixteen being saved from attempted suicide.

That, notwithstanding the continued high rate of mortality from these causes in our city, nothing special is being done to prevent it.

Lastly, we submit that in no instance can we discover that plans for the rescue or resuscitation of the drowned have anywhere been persistently carried out in the proper spirit by any municipal body; that, to be successful, such an enterprise must be wholly inspired by philanthropy.

After the reading of the above paper, it was moved by Dr. C. R. Agnew that, in view of the great importance of the paper read by Dr. Howard, it be referred to a special committee for careful consideration, the report to be presented at the next meeting. The President of the Acadamy, Dr. Peaslee, appointed on this committee, Prof. C. R. Agnew, M. D., Prof. Fordyce Barker, M. D., A. B. Ball, M. D., and Prof. Benjamin Howard, M. D., to which, by motion, was afterward added the name of the President.

At the following regular meeting of the New York Academy of Medicine, held November 7, 1872, the committee presented, through the Secretary of the Academy, the following report:

NEW YORK, *November* 7, 1872.

The committee to which was referred the paper of Dr. Benjamin Howard, entitled "The Preventable Deaths from Drowning within the Metropolitan District of New York," would respectfully report as the result of its deliberations the following memorandum of resolutions for the consideration of the Academy:

Resolved, That the insufficient provisions made within the Metropolitan District of New York to save persons from drowning call for some well-considered, vigorous, and immediate action on the part of the people.

Resolved, That this Academy will give such aid in exciting public attention and organizing public effort as the importance of the subject invites, and the functions of the Academy will allow.

Committee.
C. R. AGNEW,
FORDYCE BARKER,
E. R. PEASLEE,
A. B. BALL,
BENJAMIN HOWARD, *Secretary.*

Dr. E. R. Peaslee, the President, wished to repeat, with great emphasis, the fact, as shown in the paper of Dr. Howard, that efforts for the rescue and resuscitation of the drowned had nowhere in the world been successfully carried out by any municipal organization. He desired to call the attention of the Academy especially to this fact that all past experience in other countries had shown that voluntary organizations alone had been successfully maintained for this object. In this city municipal efforts had been made, and had in a short time utterly failed; now the only thing which remained was the formation of some society for this purpose, corresponding more or less to those which had yielded such noble results in other countries.

On motion, the report of the special committee was unanimously adopted.

An additional motion was then made by Dr. A. C. Post, and adopted by the Academy, that the committee be continued, and be further requested to report to the Academy such measures as they deem best calculated to carry out the suggestions of Dr. Howard's paper.

Bibliographical and Literary Notes.

ART. I.—*Thermic Fever, or Sunstroke.* By H. C. WOOD, Jr., M. D. Philadelphia: J. B. Lippincott & Co., 1872.

THIS exceedingly interesting memoir is the result of a series of clinical observations and experiments, undertaken with a view to determine the nature as well as the cause of sunstroke. The work commences with the history of the disease, and the author gives an interesting and complete analysis of symptoms noticed by other writers and observers.

He sums up his review with the following brief sentence in reference to what he prefers to consider "sunstroke *sui generis:*"

"The truth seems to be that all the minor symptoms vary, that the important characteristic and always-present symptoms of this variety of *coup de soleil* may be summed up as intense fever, with profound disturbance both of the cephalic and spinous nervous system; the disturbance manifesting itself in the form of insensibility, with or without delirium, and with restlessness, convulsions, or paralysis of the motor tract."

The second part concerns the nature of "Insolation."

"Sufficient of authority and reason has been brought forward to make it at least probable that heat is the sole exciting cause of sunstroke. This being so, it is to be expected that the lower animals, as well as man, should suffer from the affection, and experience fully corroborates this *a priori* reasoning. . . . A discussion of the nature of *coup de soleil* must, I conceive, rest largely upon such basis of experimentation."

We do not agree that heat is the sole cause of sunstroke, and wish that this statement was shown by Dr. Wood, to be the result of *his* experience, rather than presented as an axiom. If he would mean *all* animals are liable to sunstroke when exposed to the same temperature, there might be found persons who would dispute this, unless supported by facts. He has, however, confined his experiments principally to rabbits and cats, and secondarily to pigeons and dogs. Rabbits and cats certainly are convenient animals upon which the physiologist may conduct experiments, and so also are pigeons and dogs, but we should not lose sight of the fact that the lower animals most generally are protected by hair and feathers, that make them less susceptible than man to the direct solar rays. We should also remember that pigeons are often subjected in stable-lofts to the heat of the sun, and this too when the thermometer may indicate a temperature of 120° to 130° Fahr.

"The symptoms produced by exposing an animal to excessive heat do not appear to differ save in degree, whether the heat be artificial or due to the direct rays of the sun, or whether it be moist or dry. The animal is at first excited, trying to get away from the cage in which it is confined. This period of excitement sooner or later, according to the intensity of the heat, gives place generally to a second stage of profound muscular prostration and quietude. From the beginning, the respiration has been exceedingly hurried, and now very often it cannot be counted. The beat of the heart keeps pace with the respiration, and panting and exhausted the animal lies quiet, with the saliva pouring from its open mouth. This second stage soon yields to that of coma. In my own experiments the insensibility has generally come on gradually, not suddenly, and has not been accompanied by convulsions. In one case, however, the animal was attacked by coma and fatal convulsions with absolute abruptness."

The animals upon whom the experiments were conducted were confined in a box having a slanting glass lid; but we are not informed that precautions were taken to see that the animals were allowed plenty of pure air. This is rather important, for there might be found persons who would attribute the disturbance of respiration, the convulsive movements, coma, and death, to a diminished supply of atmospheric air. It may be that Dr. Wood took proper precautions in the construction of his "hot-box." An experiment is recalled to our mind, wherein a rabbit showed marked distress in respiration when confined in a large bell-glass, simply laid upon a rough board.

The accounts of the autopsy, in the experiments with the hot-box, accord with autopsy of persons and animals suffocated in a non-oxygenated atmosphere. The condition of the heart is also quite similar to what is noticed in ordinary asphyxia. The increase in temperature may, perhaps, be due to the same cause. For fear that Dr. Wood may think we have made an unjust criticism of his experiments, it has occurred to us to try the following simple experiment: We placed a pigeon in a bell-glass (having a capacity of about one gallon), and placed this upon a common board. In half an hour afterward the pigeon seemed in great distress for breath, which increased until at the end of two hours she was gasping, panting, slobbering, and apparently unconscious. Unfortunately she was left unobserved for an hour longer, at which time (three hours from the commencement of the experiment) she was found dead, cold, and perfectly rigid, lying in such a position as to preclude any probability of convulsions. This experiment, we agree, is quite rough, but is not so very unlike those related by Dr. Wood on page 44. We admit that his pigeons lived but an hour in the hot-box, but it should be observed that, in our experiment, some air passed in between the edge of the glass and the board upon which it was placed; again, our experiment was performed in November, in a place where the thermometer registered below 60° Fahr., while those of Dr. Wood were at a temperature twice as high. We do not relate this experiment or criticise those of Dr. Wood with any malice, but simply to note that all the conditions under which experiments are made should be reported in every detail possible; nor at the same time do we ourselves desire to be credited with a carefully-recorded experiment. The muscular rigidity upon which Dr. Wood lays so much stress, and the cause of which he explains by according assent to the proposition "that thermic and *post-mortem* rigidity are alike due to the coagulation of a plasma in the muscle," is hardly to be accepted without further investigation. Dr. Wood does not give due weight to a number of experiments made by Brown-Séquard upon arterial and venous blood, and which were reported in the *Journal de Physiologie*, 1858, pp. 95–122. On page 115, Experiment IX., M. Brown-Séquard, by a ligature around the abdominal aorta, prevented the supply of oxygenated blood to the lower extremities, which were rigid two hours after the ligature was secured; and, when the ligature was then removed, muscular irritability was reëstablished in the lower extremities. And again, (page 731), M. Brown-Séquard *proposes:*

"That muscular irritability can apparently be maintained for an indefinite period, in limbs separated from the body, and into which oxygenated blood has been injected."

And again, page 122: "Venous blood is absolutely incapable of regenerating the vital functions of muscles, when once these have completely disappeared;" while he asserts that Dr. Kay could not have succeeded in reëstablishing muscular irritability by the injection of venous blood, if he had waited until this irritability had entirely disappeared.

This theory of Dr. Brown-Séquard, fully supported by Stannius, Dr. Wood ingeniously (for he is a clever writer) attempts to twist into a confirmation of the plasmic theory (page 57). The argument he uses is that the warm alkaline blood redissolves the coagulated plasma (musculin or myosin), and thus revivifies the muscular irritability. The experiments of Kühne, quoted by our author, relate only, as it seems to us, to the chemical character of myosin, and leave entirely out of account its physical properties. We do not see the force of the argument (page 58): "If the contraction were a vital phenomenon this (vital action) could not be, because the muscle should relax as soon as dead, and, not being dead until relaxed, while still rigid, ought to be capable of being restored."

Dr. Brown-Séquard does not support Dr. Kay's theory to its full extent, but lays out a table, showing the length of time after the complete loss of muscular irritability (half an hour after its apparent abolition) that the different nervous and contractile organs can recover their vital properties under the influence of blood charged with oxygen.

This eminent physiologist asserts (page 732) that the injection of serum into organs that have completely lost their irritability gives negative results in regard to the regeneration of their vital properties; and, moreover, states that the injection of saline solutions into nerve-tissues does not reëstablish their vital functions. This question he discusses at considerable length, and states that (page 733)—

"In employing mixtures of serum and of defibrinated blood, charged with oxygen, he has proved that the larger the proportion of blood, and consequently of oxygen, the greater the power of regeneration," and concludes (page 735)—

"That a tissue cannot be dead when it has lost its vital properties or spontaneous activity for one or several hours, for their vital properties or activity can be reproduced by the aid of blood charged with oxygen."

On page 59 Dr. Wood, having assumed that the muscular rigidity after thermic fever is due to coagulation of myosin, very graphically describes, in a translation from Vallin, its march through the muscles of the body, commencing first with the left ventricle, effacing its cavity, and hence explains the relative arterial anæmia which all the organs present, and "the right ventricle becomes rigid more slowly and irregularly, and preserves in some cases for a time its activity, so that the blood of the pulmonary circulation, not finding access to the left ventricle, accumulates in the lungs, producing an enormous congestion, and sometimes rupture." This is certainly an ingenious theory, but hardly supported by experiments, and, as Dr. Wood justly remarks, "the testimony of Vallin must be received with guardedness, as he is so fully committed to the support of a theory."

On page 61 our author writes: "From these different views of the subject it may be safely inferred that in some cases, where an animal is killed by overheating, the temperature reached during life is that at which myosin removed from the body coagulates, but in others it is a degree or two *below the point!*"

In these latter cases, then, patients do not die from coagulation of the myosin, but still we are invited to believe that the heart has a special elective action above that of other muscles upon the coagulation of myosin.

Is the coagulation of myosin an instantaneous result of overheating?

Experiment XII. may be viewed in two ways: First, in the way our author interprets it; and, again, it may be that the heat and confinement of a close box brought on a vascular congestion, which was not relieved by immersion in cold water; in fact, the pigeon could not get up a reaction after his Turkish bath.

Dr. Wood does not feel quite content with the coagulation theory, but inclines to adopt that as one cause, and asphyxia as the other, and expresses his proposition in these words (page 64):

"Death in the lower animals from sunstroke or heat-fever, not produced from a sudden intense heat, occasionally is the result of coagulation of the cardiac myosin; but in many instances, and probably in the majority of cases, is not so produced, but is the result of arrest of respiration."

It is hardly just for him to state this proposition, considering the small amount of evidence adduced in its favor.

Next, our author considers the effect that muscular exercise has upon the power of coagulating myosin, and for this purpose has used frogs, cold-blooded animals; and, from a general review of certain experiments (not related in detail), he states another proposition, calling in to its support the testimony of Dr. Harless, viz.:

"That long rest of a muscle diminishes the tendency of myosin to coagulate."

The experiments of Dr. Harless related to poisoning by strychnia, while those of Dr. Wood related to frogs taken in a torpid state, the latter being an attempt to prove by negation the above proposition. We should hardly be willing to accept this last proposition as proved, if due consideration is made to the excellent Croonian Lecture given by Dr. Brown-Séquard, starting with this proposition as incontestable. Certain phenomena and observations are ingeniously fitted in, to add strength to what at present we must consider a fallacious proposition.

It is but just, however, to give our author's conclusions, though we by no means give them our adherence:

"The prolonged study of the muscular system in isolation has, then, produced the following conclusions:

"1. Excessive rigidity of heart, due to a coagulation of its myosin, is a very pathognomonic lesion of sunstroke.

"2. That in most cases it is a *post-mortem* rather than an *ante-mortem* phenomenon, occurring directly after death.

"3. In certain cases, the so-called cardiac variety of sunstroke, death is probably due to a sudden *ante-mortem* coagulation of the cardiac myosin, and consequent instantaneous arrest of the heart's action.

"4. That the muscles after death from heat-fever very soon become rigid, sometimes do so instantly, and that such rigidity is of the same nature as ordinary *post-mortem* rigidity.

"5. That while it is conceivably possible that death from asphyxia may occur from coagulation of the myosin of the diaphragm and other respiratory muscles, it is exceedingly probable that in man death never does actually occur from such cause."

To complete his study of the nature of sunstroke, Dr. Wood has repeated certain experiments originating with Vallin, upon heat as applied directly to the brain by means of a hot-water bonnet, which seemed to produce unconsciousness, sometimes convulsions, and resulted in death, unless cold water was substituted for the hot water, and in two of the former the brain-mass indicated immediately after death a temperature of 114° to 115° Fahr. In a pigeon, killed by asphyxia, we have noted a temperature in the thoracic viscera of 114° Fahr.; so that we feel obliged to take exception to his conclusion (page 76) that "a temperature of the brain of from 113° to 117° Fahr. is sufficient, if maintained, to produce death in a short space of time in mammals by arrest of respiration." Perhaps the arrest of respiration may cause an elevation of the animal temperature.

Dr. Wood seems to have overlooked the fact that the normal temperature of the various mammals differs, it being in rabbits from 99° to 104° Fahr., in cats from 101° to 102° Fahr., and again in pigeons from 107° to 110° Fahr. Variations in the degree of animal temperature are certainly valuable signs to the physiologist, but it is hardly safe to call them causes of disease and death; therefore, though there may be a "lethal nerve-heat point," this may be the effect of organic disturbance without being its cause. For these reasons attention should be called to certain sentences in which our experimenter, in his virtuous but dangerous zeal, has been induced to express, perhaps, more than he really meant; on page 83, for example—"For reasons before given, it seems irresistibly probable that if a temperature of 108° Fahr. in the brain of a cat will produce these" (coma and convulsions) "serious symptoms, it will induce no less in man." We do not see that the three facts, referred to on page 84, render the congestion theory invalid; we should be disposed to read these facts somewhat in the following way:

1. Congestion of the brain does sometimes cause epileptiform convulsions, and so do also asphyxia and certain neurasthenic poisons.

2. Opening the skull through the longitudinal sinus does not always relieve a congestion of the brain-mass, and Dr. Wood remarks in the autopsy of a tom-cat, in Experiment XVI., "There was a decided but not extraordinary congestion of the brain."

3. Pouring of cold water, or affusion sometimes called, has been supposed to relieve congestion by a stimulation of the small vessels, and thus might also relieve its concomitant coma. The physiological function of the nerve-cells is not yet completely understood, and, therefore, we do not know whether stimulation or enervation of these cells would produce the effects described by Dr. Wood; therefore his logical objection to Dr. Richardson's theory of the influence of cerebral circulation upon the nerve-tissue in sunstroke is hardly well taken.

It may seem as if too much has been said about this memoir on sunstroke, but we cannot forbear from criticising that portion of the work relating to changes in the blood so summarily disposed of on page 93; nor does Dr. Stiles's experiment disprove the theory that the blood of an animal destroyed by sunstroke has not undergone a change. There are many experiments recorded and known to experimenters in which the blood of an animal, fatally poisoned by drugs, has been injected into the blood-vessels of another animal which has not succumbed to its baneful effect. It is putting out of sight all idea of a physical change in the blood of the first animal.

The severe discipline undergone by the rabbit and pigeons in Experiments XXVI., XXVII., XXVIII. and XXIX., does not prove what Dr. Wood claims, viz., that animals poisoned by exposure to an excessive temperature have been relieved by a withdrawal of unnatural heat, and it is not strange that only one of these recovered. It may be interesting for Dr. Wood to know that if a rabbit is shaven, and its skin made impervious, by varnish, oil, glycerine, or collodion, its animal temperature becomes abnormally elevated, and death is the result. This is a well-known experiment among physiologists.

In regard to treatment we have one word of comment, and there our criticism will cease, though we do not feel that all has been said that might be advanced in opposition to his belief that simple exposure, without undue physical work, to a certain temperature, will produce thermic fever simply by elevating the animal heat. We would not be willing to put cause for effect. Who has not with perfect immunity been subjected in a Turkish bath to a temperature of 120° to 145° Fahr. for an hour, and longer too, without having an attack of thermic fever? Dr. Wood speaks of the great value of bleeding in insolation. Cannot this value be explained by the relief of congestion of the nervous centres? The interesting and useful account of the case of Dr. Mitchell seems in accordance with this theory.

We cannot let Dr. Wood off without one word of advice. When quoting the words and reviewing the works of others he should give them not only credit for what they really say, but should also be particular in giving an accurate reference to their memoirs or articles. This we have observed in only a few instances—Dr. Morgan's work on "Electro-physiology" (page 68), and when giving a history of "Clinical Records," in Part I.

We ask of Dr. Wood and of our readers pardon for taking so much space, and for being apparently a severe critic; but we deem it of great importance, in physiological researches, that due care in experimenting and in theorizing should be exercised.

ART. II.—*On Winter Cough—Catarrh, Bronchitis, Emphysema, Asthma.* By HORACE DOBELL, M. D., etc. Second edition, pp. 238. Philadelphia: Lindsay & Blakiston.

THIS work is not a systematic treatise upon the abovenamed diseases, but a course of nine lectures delivered at the Royal Hospital, giving an expression of the author's views upon the relations between catarrh, bronchitis, emphysema, and asthma, and their treatment.

Nearly all of Lecture I. is devoted to proving that emphysema can only be produced by "forcible expansion of the air-vesicles during the expiratory act," taking issue against the inspiratory, collapse, and degeneration theories. The argument is mainly the same as Gairdner's in regard to the dilatation being produced by the expiratory act, but Dr. Dobell differs from that author in thinking collapse in a remote portion of the lung can enter into the process.

The causes of emphysema (Lecture II.) are any thing which obstructs the outward tide of air, and which "may *at once* or *gradually* overstretch the air-cells." Of the first class, "violent expiratory acts performed with a closed glottis," and "*convulsive* expiratory acts, as whooping-cough, nose-blowing, and the like, when of undue force and opposed by undue resistance." Of the second class, *ordinary* acts of coughing, sneezing, and nose-blowing, under different circumstances, the most potent of which is, obstruction by narrowed naso-pulmonary air-passages.

Much space is occupied in illustrating how nasal and pulmonary catarrh—"the transient flush, the more permanent turgidity resulting in secretion, the increased susceptibility to the repetition of these states, and the final hypertrophy of the membrane"—excite "spasmodic contraction of the bronchial muscular fibre" (asthma, which is only thus incidentally considered), and cause dilatation of the air-cells and bronchial tubes. The constitutional and hereditary nature of asthma and emphysema is disposed of, tables being inserted to show that *bronchitis* is probably the hereditary affection. The want of *post-mortem* changes in the lining membrane of the larger air-passages in a certain proportion of cases does not militate against the theory. Much space is also devoted to the report of (living) cases (83), and to furnishing statistical tables (23) of relative symptoms, conditions, etc.

Dr. Dobell claims to be the only writer who has given importance to the mode of production and modification of *pitch* (p. 53, *et seq.*), viz., an increase being due to a more rapid current of air consequent upon narrowing of the tubes, although Andral, Flint,[1] and others,[2] have expressed the same idea. The application is made in the differentiation of *permanent* and *spasmodic* bronchial contraction, the high-pitched expiratory sound indicating the former, but a high-pitched inspiratory sound establishes the existence of asthma. The same principle is applied in the explanation of the expiratory sound in the early stage of tuberculosis—narrowing of the bronchial tube by tubercular encroachment. We think this is not the generally-received opinion,[3] and is a difficult one to sustain.

"Ear-cough" is a name given to sympathetic cough supposed to depend upon disease of the external auditory meatus. The author seems to think the association sufficiently intimate to entitle it to a new name.

The *treatment* of winter-cough is directed to dilated right heart, collapsed lung, emphysema, dilated tubes, disintegration of lung-tissue, cough and short breath, and, what is considered of the utmost importance, catarrh and thickening of the nasal, pharyngeal, and pulmonary mucous membranes, with their susceptibilities. The author devotes over a page to the importance of the first-named indication, without giving a single direction. Six pages reiterate the causation of emphysema, when a description and recommendation of "a residual air-pump" follow. But, if "we could nip every catarrh in the bud, what a catalogue of ills we should prevent!" This is said to be accomplishable by sesq. carb. of ammonia and morphia for two days, with ℥jss liq. ammon. acet. the first night, and a comp. colocynth pill the second. In confirmed catarrh and bronchitis, in addition to medicine and hygienic regulations, winter residences in warm latitudes (the Mediterranean coasts, etc.), and a summer sojourn in high localities, are recommended according to Waldenburg;[1] also sea-voyages[2] under certain restrictions. We think well of the latter recommendation, but question the value of coast localities.[3] Of inland localities, testimony is in favor of the climate in some parts of California and South Carolina for irritable lungs. The author attaches much importance to treating cases *through the summer*, even if no cough continues.

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[1] "Physical Explorations," second edition, pp. 32, 131.

[2] Gerhard, "Diseases of the Chest," 1846.

[3] *Vide* Flint, "Physical Explorations," second edition, p. 172.

The work is calculated to impress one with the importance of early attention to the most common of complaints and their susceptibilities, and the many practical hints in this connection will be useful to all practitioners.

We are not partial to the wholesale use of capitals, nor are Americans accustomed to write equali*s*ed, paraly*s*ed, quin*s*ey, analy*s*e, cicatri*s*ation, etc., as the author spells them.

Four colored plates illustrate different stages of bronchitis and emphysema.

Art. III.—*On some Affections of the Liver and Intestinal Canal, with Remarks on Ague and its Sequelæ, Scurvy, Purpura, etc.* By Stephen H. Ward, M. D. London, etc., 8vo, pp. 260. Philadelphia: Lindsay & Blakiston, 1872.

The diseases of the *liver* treated of in this work are, abscess, cirrhosis, hydatid disease, jaundice, and functional derangements. Those of the *intestinal canal* are, obstruction, dysentery—subacute and chronic—peritonitis, and perityphlitis. A case of acute yellow atrophy of the liver is reported in addition to the diseases mentioned in the title.

There is but little in the account of these affections calculated to elicit comment, save on one or two points. In the article on *cirrhosis*, the author says it consists of inflammation of Glisson's capsule, and of the connective tissue. Although this is the most generally-received opinion, it would have seemed proper for the author to pay a little deference to those who think otherwise. Handfield Jones[1] in 1854 took the position that excessive formation of connective tissue was non-inflammatory in origin—a simple *hypertrophy.* According to Beale,[2] the changes in cirrhosis depend upon a *degeneration,* taking their point of departure in the secreting cells; the appearance of the so-called fibrous tissue being shrunken vessels and ducts. Dr. T. Grainger Stewart[3] is of the opinion that the point of departure is first shown in hypertrophy of the connective tissue. In the description of the *post-mortem* appearances in fatal cases of cirrhosis, no mention is made of extending the examination to the kidneys. We are quite skeptical as to the utility of alterative treatment, especially of mercury, in the early stages, as recommended. Indeed, we are quite disposed to doubt the probability of an early diagnosis.

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[1] Review of Waldenburg on Consumption, etc. By J. C. Reeve, M. D. *American Journal of Medical Sciences*, January, 1870, p. 180.

[2] *Vide* Walsh. Also Holden on "Ostracism for Consumption," ibid., January, 1871.

[3] *Vide* Simons, ibid., January, 1872.

Jaundice is regarded as being due to excessive secretion, suppression, and obstruction. The cases adduced in support of the suppression theory do not demonstrate to our satisfaction the possibility of the occurrence of jaundice from such cause. The author places a considerable reliance upon the symptoms indicative of "torpor of the liver," and is very free in his use of the terms "bilious" and "ex-bilious" relative to the fæces. While the bile is essential to healthy digestion, it is to a very limited extent excrementitious; and the dark color of the fæces is due but slightly to its presence. Calomel seems to Dr. Ward to be a very important remedy in all forms of functional derangements of the liver, and in jaundice, the employment of it being equally successful in each. From the attention we have given to the reported cases, we cannot see but the "facts" rather disprove the efficacy of mercury, and we believe with Flint that "there is no ground for the supposition" that it does any good. Recent physiological investigations in regard to the action of mercury on the liver are spoken of rather deridingly. Other treatment, as employed by the profession, is judiciously recommended.

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[1] *British and Foreign Medico-Chirurgical Review.*

[2] Flint, from Beale's "Archives of Medicine," No. 2, 1858.

[3] Bright's "Diseases," p. 184, *et seq.*

Under the head of *peritonitis*, the chronic form, occurring *sui generis*, receives prominence.

Aside from the few exceptions we have taken, the treatment generally recommended is in accordance with modern views, and the affections treated of are pleasantly handled, though by no means exhaustively; in fact, if we judge correctly, the work, for a special treatise, is rather superficial in all its parts. Several works on general practice exhibit more completeness of description and depth of research.

ART. IV.—*A Hand-Book of Post-mortem Examinations and of Morbid Anatomy*. By FRANCIS DELAFIELD, M. D.

THIS book will give to the English-reading members of the profession a relief from the sense of a want long felt. The need of a concise, systematic work on pathological anatomy in the English language is best known to those who commenced the study of the subject without acquaintance with German or French.

The author states in his preface that he proposes to furnish this book as a guide to those called on to perform *post-mortem* examinations, and to have it bear much the same relation to systematic works on pathological anatomy as a dissecting manual does to treatises on anatomy. The work is divided into four parts:

Part I. is devoted to the method of making *post-mortem* examinations.

Part II. to morbid conditions of the organs of the body.

Part III. to lesions found in cases of general diseases, of poisoning, and of violent death.

Part IV. to tumors.

The book is what its author has stated in his preface that it was his intention to make it.

The style is concise, sometimes producing a desire in the reader's mind for a word or so more to enable him to obtain the author's exact meaning; but, as a rule, it will be found that this terseness will enhance the value of the work for the purpose for which it is offered to the public. The matter is arranged systematically, the lesions of the organs being considered in the same order throughout the book, and the subject-matter is abreast with the knowledge of the day. We feel sure that those who do not often make *post-mortem* examinations will find in this work a valuable companion when called on to do so. To all those also who wish for a brief statement of the anatomy of the diseases of organs and of general diseases, and of the effects of poisons, together with a short article on tumors, based upon a classification derived from their minute anatomy, this book will offer the means of obtaining the gratification of the wish. We find ourselves able to say that it contains *multum in parvo.*

The book presents a creditable appearance. The type is fair. One change, beneficial, according to our view, would be to have the chapters on separate subjects always commence at the head of a page.

We notice a few errors of minor importance, which we consider to have been the result of oversight, perhaps typographical, and which in another edition will, no doubt, be rectified. Thus, the liver is said to be situated in the right hypochondriac region; but the author, a few sentences lower down, shows its left lobe to extend three inches to the left of the median line, and this error is simply one of omission and without effect.

ART. V.—*Practical Lessons in the Nature and Treatment of the Affections produced by the Contagious Diseases, etc.* By JOHN MORGAN, M. D., etc. 12mo, pp. 335. London: Baillière, Tindall & Co. Philadelphia: Lippincott & Co.

INSTEAD of covering the extensive field indicated by the title-page, this volume is simply a treatise on syphilis.

The principal point of value in the work, which has not hitherto been made prominent, is the fact that syphilis may be communicated by the vaginal discharges of an infected person in whom the disease is latent. Dr. Morgan is a *unicist,* and an advocate of syphilization, and therefore at variance with our best recent authorities. As a whole, the work is inferior to some already extant on the subject; it contains many awkward and some ungrammatical sentences, and is presented in an unsystematic manner.

Since the book was written, Lostorfer has discovered "corpuscles" in the blood of syphilitics, and Stricker[1] has confirmed his observations, but also finds them in the blood of persons reduced by carcinoma and tuberculosis. Stricker does not regard these "corpuscles" as identical with the "crypta" of Salisbury, whose paper was published in 1868.

The publishers have presented the book in pretty style, and have not endeavored to make a large book out of a small one by introducing large type and wide margins. The *pictures*, however, are no addition.

Art. VI.—*The Treatment of Venereal Diseases; a Monograph on the Method pursued in the Vienna Hospital, under the Direction of Prof. Von Sigismund, including all the Formulæ.* By M. H. Henry, M. D., Surgeon to the New York Dispensary, etc. 8vo, pp. 49. New York: William Wood & Co., 1872.

This little volume we consider of great value to every practitioner, as it contains, in condensed form, the experience of Prof. von Sigismund, who has unequalled opportunities for observation. The general directions are clear and to the point, and the prescriptions (198 in number) afford abundant opportunity for choice, nearly every emergency being provided for. A previous knowledge of the disease is implied on the part of the reader, so that the book is intended only as an auxiliary to those already extant.

Mercury enters into the composition of the majority of the prescriptions for syphilis. One point is worthy of remark: iodide of potassium, when used, is recommended to be taken an hour before meals, to avoid decomposition by the starch.

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[1] Translated by Bumstead for the *American Journal of Medical Sciences*, July, 1872.

It should be used considerably diluted. We regret that fuller directions are not given in regard to the length of time during which mercury is to be given. Tonic prescriptions are given for anæmic conditions.

BOOKS AND PAMPHLETS RECEIVED.—On the Treatment of Diseases of the Skin; with an Analysis of Eleven Thousand Consecutive Cases. By Dr. McCall Anderson, Professor of Practice of Medicine in Anderson's University, Glasgow, etc. London: Macmillan & Co., 1872.

The Microscope and Microscopical Technology. A Text-Book for Physicians and Students. By Dr. Heinrich Frey, Professor of Medicine in Zurich, Switzerland. Translated from the German, and edited by George R. Cutter, M. D., Clinical Assistant to the New York Eye Infirmary. Illustrated by Three Hundred and Forty-three Engravings on Wood, and containing the Price-Lists of the Principal Microscope-makers of Europe and America. From the fourth and last German edition. New York: William Wood & Co., 1872.

The Pathology, Diagnosis, and Treatment of Diseases of Women, including the Diagnosis of Pregnancy. By Graily Hewitt, M. D., Lond., F. R. C. P., Professor of Midwifery and Diseases of Women, in the University College, London, etc., etc. Second American from the third London edition, revised and enlarged. With One Hundred and Thirty-two Illustrations. Philadelphia: Lindsay & Blakiston, 1872.

On a New Method for Extraction of Cataract. By R. Liebreich, Ophthalmic Surgeon to St. Thomas's Hospital. Philadelphia: Claxton, Remsen & Haffelfinger, 1873.

Transactions of the Obstetrical Society of London. Vol. xii., for the Year 1870. With a List of Officers, Fellows, etc. London: Longmans, Green & Co., 1871.

Proceedings of the Pathological Society of Philadelphia. Vol. iii. Philadelphia: Collins, 1871, pp. 218.

Reports on the Progress of Medicine.

SURGERY.

1.—*The Surgical Treatment of Aneurism.* By Prof. HOLMES, Royal College of Surgeons, of England. [Medical Times and Gazette, June 22, 1872.]

In his lectures on "The Surgical Treatment of Aneurism in its Various Forms," Prof. Holmes said that the conclusions justified by present experience as to the applicability of Brasdor's operation in innominate aneurism appeared to him to be as follows:

1. That the distal ligature of the carotid alone, or in conjunction with

that of the third part of the subclavian, cannot be trusted to produce the complete consolidation of the tumor.

2. That the natural effect of the ligature of the carotid artery is to produce coagulation in that part of the sac directly connected with the mouth of that artery.

3. That this may suffice practically for the cure of the aneurism when the subclavian part of the aneurism is small and shows no disposition to grow.

4. That in other cases where the mouth of the subclavian artery is previously obliterated by impacted clot, the ligature of the carotid only may effect a radical cure.

5. That for these reasons it is better, in any case which appears to require distal ligature, to commence with the operation on the carotid alone, and afterward to consider the propriety of securing the subclavian either in its first or third part.

As to the justifiability of operating on the first part of the subclavian, the professor made the following remarks:

The ligature of the first part of the subclavian has hitherto failed, on account of the almost uniform occurrence of secondary hæmorrhage. There are dangers in the operation, viz., anatomical difficulties, and the possibility of wounding the sac or of finding the vessel diseased. Still, most surgeons would allow that if the ligature of a large artery involved little or no risk of secondary hæmorrhage, the operation might be repeated under similar indications to those which would justify any other operation of equal importance. The less or greater risk of secondary hæmorrhage depends on the possibility or impossibility of securing an artery so as to obliterate without dividing it. Now, this has been an object with surgeons since the first case in which John Hunter tied the femoral for popliteal aneurism; and the methods by which the attainment of the object has been attempted are the temporary ligature, the *presse artère* or acupressure, and the silver ligature. None of these, except the *presse artère*, has as yet afforded definite proof of success, and this only exceptionally.

A case lent by the College of Surgeons of Ireland, showing the obliteration of the femoral by this method for popliteal aneurism, proves, however, that an artery may be obtiterated and an aneurism cured without division of the vessel. The only thoroughly satisfactory case of silver ligature is one in which Mr. Holmes employed the wire on the femoral artery for popliteal aneurism. In this instance the wound healed in a fortnight, and, though suppuration occurred, the ligature was not discharged from the wound. But silver wire is dangerous, for if tied tightly it cuts the coats of the vessel more deeply, and produces more and more rapid ulceration than the silk thread. In preventing secondary hæmorrhage its use is too uncertain to be preferable to silk. Carbolized catgut, when properly prepared, may be used for the ligature of a large artery with perfect success without dividing the external coat of the vessel or interrupting its continuity, while the ligature itself is absorbed, and the wounds made over the vessels so tied often unite by first intention. This rapid union of the deep parts of the wound is a necessary condition for success.

The professor then gave the history of the use of catgut for this purpose, mentioning especially the experience and comments of Sir A. Cooper (given in the "Surgical Essays," by Cooper and Travers, and in the 12mo edition of his lectures published in 1829) and of Porta, who gives in his work nine cases in which the operation was performed by himself, and two in which he assisted others. Porta nowhere, however, contemplates the occlusion of the vessel without its division; and, speaking of secondary hæmorrhage, says that "we shall never be able to annihilate its possibility by any method, or to reduce it within the limits which we observe in the lower animals." In the case where Sir P. Crampton used a ligature of moistened catgut in tying the common iliac in 1828, if the catgut had been properly prepared, the ligature tied firmly and cut off correctly, and the wound and the patient been so treated as to produce speedy consolidation of the deep parts, there would probably have been no hæmorrhage.

After referring to Mr. Lister's tract "On Ligature of Arteries on the Antiseptic System," Prof. Holmes remarked that, whether Mr. Lister's explanation of his method of dressing wounds be or be not the correct one, that method is very frequently successful in producing rapid union, especially in the deep parts of the wound. This being granted, and as it can be demonstrated that catgut ligature, under favorable circumstances, will gradually melt away in the tissues without causing ulceration, it cannot be denied that the object which John Hunter and his followers so sought after had been discovered. A preparation of the lecturer's own case showing successful ligature of the carotid and subclavian arteries with carbolized catgut, and with no interruption of the continuity of either vessel, was exhibited. This specimen affords anatomical proof that a large artery in the human subject may be tied in such a manner that the wound may unite by first intention, and the patient never be in any danger of secondary hæmorrhage; and, further, the case shows that the catgut ligature may be removed by absorption, the vessel remaining undivided. Mr. Holmes expresses his opinion of the carbolized catgut in these words: "No material can be imagined better adapted for a ligature than catgut long steeped in carbolized oil; it is perfectly tough, perfectly flexible, and perfectly smooth. It excites no irritation, absorbs no putrescent fluid, and, though it gradually melts away, yet holds the artery firmly enough to close its tube permanently. Having used it in all large operations for about two years, I can testify to the latter fact, and I can say that I have never seen any sign of irritation produced by it, nor witnessed the escape of any of the little knots from the wound, and that I hold it to be a very much better and more convenient hæmostatic than torsion, and infinitely superior to acupressure. In fact, I have found it to possess all the comfort of the silk ligature, without its disadvantages. I do not, however, imagine that catgut or any other ligature can be applied to an artery with perfect success unless the tissues around become rapidly coagulated by first intention. An artery exposed in the middle of a suppurating cavity will, I believe, always soften and give way, and it was most probably because they failed to secure this prompt union of the deep parts of the wound that Porta in most of his experiments and operations, and Sir A. Cooper in the operations which he performed after his first successful one, failed to obtain this perfect result." The inference, then, is that the subclavian may be justifiably ligatured in the first part of its course in those innominate aneurisms which advance after distal ligature of the carotid.

The applicability of the distal operation in the treatment of aortic aneurism, and the probable reasons of its beneficial effect in this form of the disease, were next discussed. With respect to the ligature of the left carotid for aneurism of the arch of the aorta, there is no doubt that there are cases in which it is very beneficial, though there are no theorétical grounds for believing that cure can be effected by this operation. The beneficial result no doubt is produced by the obliteration of that part of the sac which was previously distended by the current of blood passing into the left carotid. When, then, the growth of the tumor can be traced upward toward the trachea on the left side, this operation is indicated, and when on the right side the distal operation on the same side will suggest itself. Finally, the treatment of aortic and mixed aortic aneurisms by galvanopuncture was considered, and the difference in the clot produced in any albuminous fluid according as the positive or negative pole is used was mentioned; the positive pole producing a firm coagulum, and the negative a large, frothy mass formed of minute coagula.

Mr. Poland's summary of Abeille's case proves that the clots produced by electricity may be as efficacious in the cure of an aneurism as those produced by other means; but it must be allowed that there is at present much uncertainty as to the value of the method, and as to the effects of any given operation, and many details as to the method of employing this agent have to be cleared up before our estimate of the value of it can be formed with certainty. Prof. Holmes is disposed to think that both poles should be used, that the action should continue for a considerable time under anæsthesia, and that the needle should be partly coated with vulcanite, so as to defend the soft parts covering the tumor. The uncertainties connected with its employment, however, are sufficient for discarding electro-puncture in cases of aneurism of arteries accessible to pressure or ligature, as these methods are safer and more reliable.

2.—*Spontaneous Gangrene of Both Feet.* By Mr. GANT. [Medical Times and Gazette, May 11, 1872.]

At a meeting of the Clinical Society of London, Mr. Gant related a case of spontaneous gangrene of both feet in a messenger-boy, aged sixteen, who, when he was about seven years old, had a decidedly bluish aspect, was always breathless, and complained of feeling cold even in warm weather. The boy himself stated that he had suffered from shortness of breath, with occasional dyspnœa, ever since he could remember; otherwise the boy's general health had remained unaffected. Latterly he had been much exposed to cold during very severe weather. After a day's exposure, during the night, when in bed, his feet "burned very much;" and the next morning the great-toe and two adjacent toes of both feet were nearly black, and quite insensible. In this state he walked to the Royal Free Hospital. After admission, gangrene progressed until it reached the dorsum of both feet, presenting, to some extent, the appearance of senile gangrene. But the livid blueness and œdema of both legs up to the knees contrasted with the pale, shrunken condition of the legs as observed in senile gangrene; and the gangrene itself, at this early stage, was more succulent or moist than dry. Pulsation was quite free in both the tibial arteries of the feet. A physical examination of the chest was made by Dr. Cockle. The heart's action was feeble but regular, and unaccompanied by any appreciable murmur. Pulsation could be readily seen in both carotids, and a diffuse forcible pulsation in the external jugular veins. Pulse at the wrist 27 per minute, small, feeble, irregular. The only pulmonary signs were emphysema, and slight bronchitis at the bases of both lungs. Respiration was oppressed —16 per minute. The temperature in the axilla was, morning 103.8°, evening 101.6°. The urine was scanty and high-colored, with a copious deposit of grayish-brown lithates and of albumen. The patient was lethargic, dull of understanding, and he spoke with a stridulous voice and broken articulation. Occasionally, when the heart's action became very feeble, and dyspnœa very urgent, fits of unconsciousness occurred, accompanied on one such occasion with epileptic convulsive movements of the upper limbs and face, this state lasting two hours. A line of demarcation having formed on the dorsum of the feet, Mr. Gant performed double amputation by modifications of Chopart's and Hey's operations. Both stumps united soundly in a week. But the cardiac and pulmonary conditions recurred at intervals with increasing severity; and in a fortnight the patient sank into a state of coma, and died quietly. The *post-mortem* examination showed that the heart in its right half was the seat of disease, the origin of which would seem to have been at a very early period of life. The right auriculo-ven tricular opening was enlarged sufficiently to admit two, or even three fingers; but the three segments of the valve were healthy; the auricular cavity was dilated and distended with blood to more than twice its natural size; the wall of the auricle, not thickened in any part of its extent, was, between the musculi pectinati, reduced to a membranous state; the foramen ovale was entirely closed; the right ventricular cavity was also somewhat enlarged, and equally distended with blood, but the wall was not hypertrophied. The pulmonary valves were quite healthy. On the left side of the heart no morbid condition was discovered, either as regards the cavities or the valves. Recent pericarditis had occurred. Both lungs were emphysematous, but the texture generally was much congested, and the bronchial tubes were engorged throughout with frothy mucus. Recent pleurisy had taken place on both sides of the chest. The abdominal viscera generally were much congested, and the liver and kidneys considerably enlarged by interstitial deposit, passing into fatty degeneration. The brain exhibited marked venous congestion. Mr. Gant was disposed to draw the following two conclusions: 1. That the gangrene of both feet, depending evidently on systemic venous congestion, was caused by the enlarged state of the right auriculo-ventricular opening; and the accompanying dilatation of the auricle and ventricle, being thus essentially cardiac, although the immediate and exciting cause might have been exposure to cold. 2. That the immediate cause of death was pericarditis, pleurisy, and more particularly capillary bronchitis.

3.—*Flap Operation for Vesico-vaginal Fistula.* By Dr. MAPOTHER. [British Medical Journal, June 1, 1872.]

At a recent meeting of the Surgical Society of Ireland, Dr. Mapother described a method of operating for vesico-vaginal fistula, proposed by Dr. G. H. Kidd, and illustrated his remarks by a case in which it had been successful. The patient, aged twenty-five, had fistula from sloughing of the anterior wall of the vagina close to the neck of the bladder. There was a small circular opening. At the beginning of March, in accordance with Dr. G. Kidd's procedure, a semicircular or U-shaped incision was made round the fistulous opening. The flap thus made was turned aside; and a second flap, formed by an incision half through the vesico-vaginal septum, having been pared on the vaginal surface, was brought up over the situation of the fistula and secured by four wire sutures. A No. 6 catheter was kept in the bladder for some time, and a rapid and complete recovery followed. The escape of urine was quite prevented by the flap being brought over the vesico-vaginal opening. Dr. G. H. Porter had seen the operation performed on one occasion by Dr. Roe. The aperture was of very small dimensions. He considered that, from the nature of the soft parts engaged in the procedure, the knife employed should be extremely sharp. Dr. Kidd looked upon a pin-hole fistula as most difficult to close. In one instance seventeen operations had been performed on the same patient for a very minute opening. He referred to Dr. Bandon's fish-hook sound or probe, in withdrawing which the operator was enabled to grasp and to divide the sides of the fistula. Dr. Kidd had operated on seven occasions in the way described by Dr. Mapother, and always with good results. In the *Dublin Quarterly Journal of Medical Science*, for February, 1871, Dr. Barton had spoken of a somewhat similar procedure. Dr. W. Roe said that the operation suggested by Dr. Kidd was applicable principally to small fistulæ. Mr. Cronyn believed that the vast majority of operations for vesico-vaginal fistula proved unsuccessful. The practical point to be deduced from a recognition of this fact was, the limitation of the duration of labor in its second stage. Dr. Fleming said that, where evil consequences followed in cases of labor, the patients had generally been attended by midwives, and not by medical men. Dieffenbach's plastic operation for urethral fistula was of the same nature as Dr. Kidd's for vesicovaginal fistula. Dr. Mapother drew attention to the analogy between the formation of the fossa ovalis from the foramen ovale, and the closure of a fistula by a flap applied on the side of an opening remote from the outpressing fluid. He also quoted Mr. Lane's case of bringing the os uteri into the bladder by a plastic operation.

4.—*The Treatment of Strangulated Hernia by Puncture of the Intestine.* [Medical Times and Gazette, August 3, 1872.]

The very great importance to surgical practice which the introduction of any plan for the treatment of strangulated hernia implies, induces us to refer to a method suggested by Mr. Bryant in a clinical lecture delivered at Guy's Hospital in the month of February of this year.

It will be in the recollection of some of our readers that, in the third lecture of his series (which appeared in our columns on April 20th), Mr. Bryant gave the details of a case which occurred to him in 1871. It was that of an old man, aged seventy-one, the subject of strangulated scrotal hernia, upon whom he had performed herniotomy, it being necessary to expose the bowel in order to return it. Owing to the restlessness of the patient, however, the gut again descended in a few hours, and all attempts to reduce it failed, until the large intestine was punctured in four or five places with a grooved needle, and a quantity of flatus (but no fecal matter) let out. The patient recovered without a single bad symptom. In his subsequent remarks upon this case, Mr. Bryant observed that it proves clearly that puncturing the intestine freely may be resorted to with every prospect of success in some cases of hernia. He raised the question whether it is not possible to reduce some herniæ without any operation at all, and he stated that he had made up his mind to try the practice in large scrotal and umbilical herniæ, and thought favorably of its employment in all cases of acute internal strangulation due possibly to bands or twists.

Since the publication of this lecture, the treatment of puncturing the bowel to remove strangulation has been followed in three cases (which are now well known), two of which have been published. The third occurred in the practice of Mr. Hulke, in the Middlesex Hospital, in the early part of May. The case was one of internal strangulation from a band, in which abdominal section was performed, and the distended small imtestine punctured (ultimately laid open) before reduction of the bulging portions could be effected, and the wound closed. This case, with Mr. Hulke's remarks, will shortly appear in our pages.

Of the two published cases, one was reported by Dr. Léon Labbé, of the Hôpital La Pitié. It was a strangulated inguinal hernia, of about eighteen hours' duration, in an old man, aged seventy. Here, as the taxis failed, puncture with the aspirator was proposed, and a No. 2 needle introduced. In eight days the patient was well. The other case was one brought before the Académie de Médecine, on May 21st, by M. Demarquay, a brief notice of which was given in the *Medical Times and Gazette* of June 1st. In this instance, after taxis had failed, a fine trocar was passed into the centre of the tumor, which was a strangulated congenital inguinal hernia, and by means of Potain's aspirator about 120 grammes of intestinal liquid were drawn off into the recipient. No ill consequence followed, and the patient rapidly got well.

Thus, then, this new mode of treating certain kinds of hernia, which has been tried and suggested by Mr. Bryant, has since found favor in English and foreign practice; and we hope, as time goes by, to be able to report favorably of it as a means of relief in those cases in which hitherto little or no success has crowned surgical efforts.

Miscellaneous and Scientific Notes.

Appointments, Honors, etc.—At a meeting of the Obstetrical Society of London, held October 2, 1872, the following gentlemen were elected Honorary Fellows of the Society: Fordyce Barker, M. D., Professor of Clinical Midwifery and Diseases of Women in the Bellevue Hospital Medical College; Otto Spiegelberg, M. D., Professor of Clinical Midwifery at Breslau; and T. Gaillard Thomas, M. D., Professor of Obstetrics in the College of Physicians and Surgeons, New York. Among the names of gentlemen elected Fellows of the Society at the same meeting, we notice that of Prof. Trenholme, M. D., of Montreal. The freedom of the city of Bologna has been conferred on Prof. Virchow, on account of his eminent services to science. Dr. C. B. Brigham, of Boston, has been decorated by the Emperor of Germany with the Imperial Order of the Crown, for his services to German and French wounded soldiers in the hospital at Nancy. Dr. W. R. Gibbes, of Columbia, has been elected Professor of Anatomy and Surgery in the University of South Carolina, in place of Dr. Darby, resigned. Prof. Agassiz has been elected one of the eight Foreign Associate Members of the French Academy of Sciences, to fill the vacancy caused by the death of Sir Roderick I. Murchison. M. Chevreul, the distinguished chemist, it is stated, has been honored by the Academy of Sciences with the gift of a medal bearing his name. M. Dumas delivered a speech on this occasion, of historical interest. M. Chevreul is in his eighty-sixth year. His courage saved the Museum of Natural History from destruction during the bombardment of Paris by the Prussians.

Holloway's Hospital.—We are informed by the *British Medical Journal* that Mr. Holloway, notorious for his pills and ointment, is about to erect, at his own expense, a large hospital at Virginia Water, near London. It will cost from seventy thousand to one hundred thousand pounds, and will accommodate two hundred patients. It will be maintained for a year by Mr. Holloway, after which it is expected to be self-supporting, and will be handed over to the management of trustees.

Suicides in England.—Suicides are reported to have increased in England, in the last six years, from an annual average of sixty-six to every million of population, to an annual average of nearly sixty-eight to the million. The annual average of suicide by drowning has increased, that by hanging has diminished.

Medical Libraries.—The largest medical library in the United States is that of the Surgeon-General's office, Washington. It has 18,000 volumes. The second in size is that of the College of Physicians of Philadelphia, nearly 16,000; and the third, that of the Pennsylvania Hospital, 13,000 volumes.

Correction.—Our readers are requested to correct an error, which was no doubt apparent to many from the context, in the lecture by Dr. Barker, on "Puerperal Mania," in the last number of the JOURNAL: on page 460, eighth line from the bottom, for "hysteria" read "dystocia."

Prize Essay on Galvanism.—The Academy of Sciences of Bologna has resolved to give in 1874 a prize of 1,200 francs—Prix Aldini—for the best scientific experimental essay on galvanism or dynamic electricity. The competition remains open till June 30, 1874.

The Astley Cooper Prize.—The next triennial award of this prize of five hundred pounds will be made to the author of the best essay on "Injuries and Diseases of the Spinal Cord." Essays are to be sent before January 1, 1874, to Guy's Hospital, London.

Vaccination in Australia.—The Assembly of South Australia has passed a bill in favor of making all medical practitioners public vaccinators, and awarding the sum of three shillings and sixpence for each successful operation.

The Pennsylvania University Hospital.—Work has been commenced in levelling and grading the lot in West Philadelphia, bounded by Thirty-fourth and Thirty-sixth and Spruce and Pine Streets, on which the new University Hospital is to be erected; $200,000 has been subscribed for the hospital, besides a grant of $100,000, which was obtained from the State Legislature. The building will be complete in all the details necessary for a first-class hospital. The new university is nearly complete, and the fall term of studies will be commenced in the new building. — *Philadelphia Medical Times.*

Naval Examining Board.—The Medical Board for the examination of candidates for admission and promotion in the medical corps of the navy, which has been in session in this city since 1868, has been reconvened in Washington, with the following officers: Medical Director Charles D. Maxwell, President; Medical Director William Grier, Medical Inspector Thomas J. Turner, Surgeons Albert L. Gihon and Richard C. Dean, members; and Surgeon Joseph Hugg, Recorder.—*Philadelphia Medical Times.*

Jefferson Medical College.—We are informed that the project of a hospital in connection with this school is being fostered with much energy. No one as yet knows, we believe, whether the trustees will buy up properties adjacent to the present college building, and erect the hospital there, or whether they will transfer the school to some other point. Both plans are spoken of, and each seems to us to have certain advantages.—*Philadelphia Medical Times.*